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## History of Mathematics: Models and Visualization in the Mathematical and Physical Sciences

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**ABSTRACT.** This workshop brought together historians of mathematics and science as well as mathematicians to explore important historical developments connected with models and visual elements in the mathematical and physical sciences. It addressed the larger question of what has been meant by a model, a notion that has seldom been subjected to careful historical study. Most of the talks dealt with case studies from the period 1800 to 1950 that covered a number of analytical, geometrical, mechanical, astronomical, and physical phenomena. The workshop also considered the role of visual thinking as a component of mathematical creativity and understanding.

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### Introduction by the Organisers

The idea for this workshop came up in discussions at the history of mathematics workshop held at MFO during the week of March 3–9, 2013. Its aim was to bring a wide range of experts together in order to explore important historical developments connected with models and visual elements in the mathematical and physical sciences. Speakers focused on a number of case studies that dealt with visualizing geometrical, mechanical, astronomical, and physical phenomena during the period from roughly 1800 to 1950. Several talks discussed how visual models have functioned within purely mathematical disciplines. But just as many dealt with cases in bordering fields that employ mathematical theories and methods to study various physical phenomena.

A number of talks dealt with model-making in geometry during the latter half of the nineteenth century. Source materials describing the artefacts from this time, many on prominent display at the MFO, are quite plentiful. One can find much information about such string and plaster models from the catalogues of companies that produced them as well as the exhibition catalogues produced when they were put on display (South Kensington 1876, Munich 1893, etc.). More challenging for historians, however, is to understand the motivations behind this model-making activity. In most cases, the geometers who promoted it were teaching at the higher technical schools rather than at universities. Several were lesser known amateurs, whose work has been forgotten once the commercialization of geometrical models led to the proliferation of canonical artefacts.

Many speakers took note of the fact that the explicit use of the term model and/or modelling was not part of the original vocabulary of the actors themselves. Thus, the history of non-Euclidean geometry took an important turn with the work of Beltrami, Klein, and Poincaré. Yet none of these figure referred to “models” that they invented and which aimed to show the validity of the theories of Lobachevsky and Bolyai. Clearly, that terminology was taken up soon afterward, but not in their original publications. Likewise, in cosmology, the famous “models of Einstein and de Sitter” were originally referred to as “worlds”. It seems likely that the term cosmological models did not become current until 1933, when H. P. Robertson used it in a widely read review article. These and other instances suggest that much of the retrospective literature has projected the terminology of mathematical modelling into earlier work, thereby distorting our view of its intentionality.

Philosophers of science have long been interested in the role of models in theory formation, whereas historians of mathematics have seldom paid close attention to the ways in which theoretical concerns are often entangled with concrete modelling activity. This workshop thus provided a welcome opportunity to explore the relationship between different representations of a phenomenon and their role in explanation. The year 1950 marks a natural boundary line for historical studies, since after then modern electronic computers opened vast new possibilities for mathematical modelling and visualization in the mathematical and physical sciences. In recent decades computer graphics have revolutionized the once largely static realm of visualizable mathematics. Models and simulations of complex phenomena have become so commonplace that one easily recognizes how radically different things were before the onset of the IT era. By looking at particular historical contexts and special cases, the workshop offered a clear sense of how models and visual thinking developed and reinforced one another. The diverse topics reflected in the abstracts below provide at least a provisional picture of how models and visual thinking shaped important historical developments.

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## Abstracts

### A Plea for Actor's Categories: On Mathematical Models, Analogies, Interpretations, and Images in the 19th Century

MORITZ EPPLE

The rise of the notion of mathematical models is still not fully understood from a historical point of view. Comparing two famous quotes of the late 19th century by Heinrich Hertz (from his *Prinzipien der Mechanik*, 1894, p. 1-2: “Wir machen uns innere Scheinbilder oder Symbole der äußeren Gegenstände, und zwar machen wir sie von solcher Art, dass die dennotwendigen Folgen der Bilder stets wieder die Bilder seien der naturnotwendigen Folgen der abgebildeten Gegenstände. [...] Ist es uns einmal geglückt, aus der angesammelten bisherigen Erfahrung Bilder von der verlangten Beschaffenheit abzuleiten, so können wir an ihnen, wie an Modellen, in kurzer Zeit die Folgen entwickeln, welche in der äußeren Welt erst in längerer Zeit oder als Folgen unseres eigenen Eingreifens auftreten werden [...]”) and by Ludwig Wittgenstein (from his *Tractatus*, 1921, Proposition 2.12: “Das Bild ist ein Modell der Wirklichkeit”) we can see that while Hertz still conceived models as concrete, material objects in the tradition of the 19th century, Wittgenstein's early philosophy identified abstract images with models in thought.

Of course this does not imply that abstract representations of mathematical relations were absent in scientific discourse and practice in the 19th century. Indeed a variety of terms denoting such representations was used and discussed by mathematicians and physicists before the turn of the century. The talk briefly analyzed the following terms in this role: “analogies,” “interpretations,” “images/Bilder,” and “systems.”

1. In his paper “On Faraday's Lines of Force” of 1856, James Clerk Maxwell advocated the use of “analogies” in physical science. In a situation which, for a large part of electrical science, the development of “physical hypotheses” was problematic, physicists had “to obtain physical ideas without adopting a physical theory.” A tool for achieving this goal were “physical analogies. By a physical analogy I mean that partial similarity between the laws of one science and those of another which makes each of them illustrate the other” ([1], 156). The point here was the similarity of the mathematical form of two sets of laws, and the possibility of mutual illustration of one domain by the other. These analogies thus involved translations of the essential terms and a dictionary. The notion of “analogies” was taken up by many authors in various forms, including “mechanical analogies” (where one domain of the analogy is mechanics), “dynamical analogies” (linking to dynamical theory in the sense of the 19th century), etc. The epistemic roles of such analogies was varied: they had heuristic value and helped to escape the dilemma of uncertain “physical hypotheses,” thereby also taking a step back from the need to give realistic causal explanations of phenomena. Analogies were building epistemically symmetric bridges between concrete imaginations of different domains. The emphasis on shared formal properties of physical laws and relations in such

domains underpinned a tendency to create a picture of unified mathematical form in the manifold of physical phenomena.

An interesting twist to these epistemic roles was made in a well-known analogy used by Hermann v. Helmholtz in his researches on vortex motion, where the stationary irrotational flow around a system of vortex tubes was compared with the magnetic field around an equivalent system of electric currents [2]. Here the analogy was used to illustrate certain mathematical objects: multi-valued potential functions: “Ich werde mir deshalb im Folgenden öfter erlauben, die Anwesenheit von magnetischen Massen oder elektrischen Strömen zu fingiren, blos um dadurch für die Natur von Functionen einen kürzeren und anschaulicheren Ausdruck zu gewinnen, die eben solche Functionen der Coordinaten sind, wie die Potentialfunctionen oder Anziehungskräfte, welche jenen Massen oder Strömen für ein magnetisches Theilchen zukommen” ([2], 27).

2. The 19th century development of non-euclidean geometry has often (anachronistically) been described as a development of “mathematical models” (better treatments warn their readers about this anachronism). However, the geometers of the 19th century did not talk about “models.” A famous term introduced by Beltrami was the “interpretation” of a “system” of principles of geometry (in his “Saggio di interpretazione della geometria non-Euclidea” of 1868). Felix Klein, in his early papers on the topic, took up Beltrami’s notion and complemented it with the term “Bild,” a term that soon became common especially in German contributions to the area. When Beltrami’s first paper appeared in print, the situation of non-Euclidean geometry was rather unclear on an epistemological level: If geometry was supposed to describe the structure of physical space then at least one of the systems of geometry could not be true in a realistic, physical sense. (Many later philosophical, mathematical and physical publications show that this worry remained until the end of the century.) In this situation Beltrami looked for mathematical objects in traditional (differential) geometry that exhibited relations that were (at least partially) similar to those of hyperbolic geometry. Taking his clue from well-known surfaces of constant negative curvature in 3-dimensional Euclidean space, he introduced a new notion of an (abstract) surface of constant negative curvature, represented by the interior of an “auxiliary circle,” and summarized (in the French translation by Hoüel): “ces théorèmes [de la Planimétrie non euclidienne], en grande partie, ne sont susceptibles d’une interpretation concrète que si on les rapporte précisément à ces surfaces, au lieu du plan, comme nous allons démontrer tout à l’heure avec détail” ([3], 259). As in the case of physical analogies, there was a translation of terms from hyperbolic geometry to the geometry on “the” surface of constant negative curvature, and from the latter to the interior of the auxiliary circle, and again there was a dictionary for passing between the stages of this double representation. It is interesting to note that Beltrami was also interested in (actual, material) models of hyperbolic geometry, as detailed in the contribution of Rossana Tazzioli to this conference. Another similarity to physical analogies was that – rather than providing an abstract model –

Beltrami's "interpretation" added concrete imaginations of familiar mathematical objects to a not-so-familiar, abstract system of geometrical principles.

Klein rendered Beltrami's "interpretation" by the German "Bild." For him, the crucial role of "Bilder" was "Versinnlichung," i.e. to make the ideas of non-Euclidean geometry accessible to intuition (which in turn was understood as a faculty close to the senses). In the decades following 1868 a series of other such "images" of non-euclidean geometry were developed by Klein, Poincaré, Killing and others. The various relations of "Abbildung" between them became a favorite topic for followers of Klein (and others), again emphasizing epistemic symmetries between these "images."

3. In an unpublished essay entitled "Nichteuklidische Geometrie" (probably 1903) Hausdorff addressed the plurality of (new and old) geometries as "systems" (inspired by Hilbert's *Grundlagen der Geometrie*): "Unter einer einzelnen nichteuklidischen Geometrie verstehen wir jedes System geometrischer Sätze, das in irgend einer mehr oder minder belangreichen Beziehung von einem bestimmten System, der euklidischen Geometrie abweicht; nichteuklidische Geometrie, als mathematische Disciplin, stellt sich die Prüfung und vergleichende Betrachtung aller dieser einzelnen Systeme zur Aufgabe" (p. 4). He was, of course, well aware of the epistemological implications of the plurality of these systems, and conceived a new role for "interpretations" and "images" as discussed by earlier geometers, pleading for "radicalism" in the free use of language for making visible "Zusammenhänge zwischen scheinbar entfernten Gebieten" (p. 10). At the same time the "intuitions" evoked by certain images no longer carried epistemological weight in his view: "das Wort 'anschaulich' [bedeutet] zu vielerlei und eigentlich bei Jedem etwas Anderes" (p. 12). For Hausdorff, "Modelle" still referred to material objects.

4. Whereas Klein's early writings never used the term "Modell" to denote the interpretations and "Bilder" of non-euclidean geometry, the posthumous *Vorlesungen über nichteuklidische Geometrie*, rewritten and published by Walter Rosemann in 1928, did so in a few places. A possible source of inspiration for this new use is Hermann Weyl's *Raum – Zeit – Materie* of 1918 (thanks to Erhard Scholz for this suggestion). In his book on mechanics, Hertz introduced a technical notion of "model" as an equivalence relation between "material systems", still far from the modern use of the term (see Jesper Lützen's contribution to this conference.)

For the widespread use of the term 'mathematical models' in the modern sense, particular attention needs to be paid to the uses in mathematical economics since the 1930's, see [4] and [5].

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## Models and Visualization in Heinrich Hertz's Principles of Mechanics

JESPER LÜTZEN

Heinrich Hertz's book *Die Prinzipien der Mechanik in neuem Zusammenhange Dargestellt* (1894) was innovative in its approach to physics (a mechanics without forces as basic notions), mathematics (a differential geometric form) and philosophy (the theory of images). I have dealt with all these novelties in my book ([4]). Here I shall discuss three special points related to the subject of this meeting: 1. Hertz's image theory as a precursor of the modern idea of a model. 2. Hertz's own use of the word "model" and 3. the visual element in Hertz's mechanics and his earlier lecture on the constitution of matter.

### **Hertz's image theory as a precursor of the modern idea of a model.**

According to Hertz "we form ourselves Images or symbols of external objects". These images must satisfy the following basic requirement: "The necessary consequents of the images in thought are always the images of the necessary consequents in the nature of the things pictured". This requirement is very close to our present notion of a model: If a natural (or social) phenomenon is modelled by a mathematical system (for example a system of differential equations) then it should be possible to translate the solutions of the mathematical system back into the natural realm in such a way that it tells us what nature does. Thus Hertz's images and modern models share this requirement of predictive power. To be sure Hertz's images are mental images, whereas modern models are mathematical. But the difference is less marked when we notice that Hertz's images are expressed in a geometric form. For example, Hertz expressed his mechanics in a differential geometric form.

Moreover, Hertz required his images to be "permissible" (logically consistent). When an image satisfied the basic requirement above Hertz called it "correct". His experience as a working physicist had taught him that one could have several permissible and correct images of nature (for example the Maxwellian and the Weberian image of electro-magnetism, at least before Hertz's own experiments). In such a case Hertz required that one chose the most distinct image, i.e. the one that "pictures more of the essential relations of the theory" and if there are several equally distinct images, one should chose the simplest i.e. "the one which contains, in addition to the essential characteristics, the smaller number of superfluous elements". He did not believe one could obtain a well-rounded law-like image without introducing invisible things (inessential elements or idle wheels) but he required that their number be minimized.

I wish to emphasize one important way in which Hertz's images are a precursor of modern models, namely in its lack of ontological commitment (see also [5]):

“The images which we here speak of are our conception of things. With the things themselves they are in conformity in one important respect, namely in satisfying the above-mentioned requirement. For our purpose it is not necessary that they should be in conformity with the things in any other respect whatever. As a matter of fact, we do not know, nor do we have any means of knowing, whether our conceptions of things are in conformity with them in any other way than this one fundamental respect” ([3], 2)

So as in modern mathematical models Hertz did not pretend that his images would tell the truth about the inner working of nature. It is indicative of this loss of ontological commitment that in the first drafts of the book Hertz spoke of truth (Warheit) but changed it to correctness (Richtigkeit) in the later drafts.

A similar distinction between the truth about nature and our theories about nature can be found in earlier thinkers, in particular Kant, and has its roots in antiquity. Indeed, according to Aristotle, a mathematical description of nature can save the phenomena but only philosophy deals with matter and causes. Medieval thinkers upheld a similar distinction. That is why Copernicus’s system was not condemned by the church in the beginning, but only when Galileo began to argue that it was not only a mathematical way to save the phenomena but told the truth about the world. In the 17th century Descartes invented the term physico-mathematical science to denote a science where mathematics is used to determine the causes in nature. It was within this ontologically more committing trend that his successors searched for the laws of nature. ([6])

Hertz’s theory of images presents a step back to the antique and medieval ideas of mixed mathematics in the sense that they agree that the mathematical description or image does not necessarily reveal the true working of nature. But in another important way Hertz differed fundamentally from predecessors: In antiquity and the Middle Ages scholars believed that there were other means (philosophy and theology) to find the true causes in nature. Hertz explicitly rejects that: “As a matter of fact, we do not know, nor do we have any means of knowing, whether our conceptions of things are in conformity with them in any other way than this one fundamental respect”.

So for Hertz the formation of an image is the best way to understand nature. A similar idea seems to be behind a claim in the Wikipedia article about mathematical models where the author writes that “the purpose of modeling is to increase our understanding of the world”.

**Hertz on “models”.** Hertz used the word model in a way similar to his contemporaries but gave it a precise meaning within his own image of the world. In this image a physical system is a mechanical system consisting of ordinary masses and hidden masses, that we cannot sense directly. The point masses of the system interact through connections that can be expressed in terms of first order linear homogeneous differential equations in the generalized coordinates expressing the configuration of the system. Moreover, Hertz introduced a metric (or a line element) which is a quadratic differential form in the generalized coordinates. It expresses the magnitude of an infinitesimal displacement of the system. The

motion of a mechanical system could then be described by Hertz's law of motion: "Every free system persists in its state of rest or of uniform motion in a straightest path". Here "straightest" is defined relative to the metric.

In terms of these fundamental notions Hertz defined a "model" as follows:

"Definition: A material system is said to be a dynamical model of a second system when the connections of the first system can be expressed by such coordinates as to satisfy the following conditions:

- (1) That the number of coordinates of the first system is equal to the number of the second
- (2) That with a suitable arrangement of the coordinates for both systems the same equations of condition exist
- (3) That by this arrangement of the coordinates the expression for the magnitude of a displacement agrees in both systems" ([3], 418)

In contrast to our modern idea of a mathematical model, Hertz's models are images of mechanical systems. Moreover for Hertz "being a model of" is an equivalence relation, so in particular if A is a model of B then B is also a model of A. This also distinguishes Hertz's concept of a model from his concept of an image. Nevertheless, Hertz himself pointed out the similarity of the two concepts:

"The relation of a dynamical model to the system of which it is regarded as the model, is precisely the same as the relation of the images which our mind forms of things to the things themselves. For if we regard the condition of the model as the representation of the condition of the system, then the consequents of this representation, which according to the laws of this representation must appear, are also the representation of the consequents which must proceed from the original object according to the laws of this original object. The agreement between mind and nature may therefore be likened to the agreement between two systems which are models of one another, and we can even account for this agreement by assuming that the mind is capable of making actual mechanical models of things, and of working with them." ([3], 428)

This is Hertz's only psycho-physical remark in the book

**Hertz on visualization.** Hertz first advanced a theory of images in a series of lectures (1884) on the constitution of matter ([1]). Here he defended himself against philosophers who would argue that one cannot make images of matter on the atomic scale without attributing properties to the ether and the atoms that they cannot have. "Every sensible (sinnliche) image of the atoms includes an absurdity, any transfer of perceptible properties of matter to the atoms contains a logical mistake". While agreeing in principle, Hertz pointed out that: "It is a general and necessary property of the human mind that we can neither intuitively represent nor conceptually define things without attributing properties to them that do not at all exist in them". He maintained that this does not present a problem as long as we carefully distinguish the things that belong to nature from the things we have add in order to imagine nature:

"Thus let us guard ourselves from believing that we can investigate the nature of the things themselves by considering the atoms; let us also guard ourselves from



confusing the unnecessary properties that we must necessarily ascribe to them, with the essential properties that are merely time and space relations. However, let them [the philosophers] not make us believe that we have worked in vain when we have made ourselves images [Bilder] of the things that are real but do not enter into our mind, images that correspond to those things in some respects, while in other respects they bear the imprint of our imagination. We have then, in our field, followed the general course of the human mind.” ([1], p. 36)

In this way Hertz defended the formation of very colorful vivid mental images of nature. However, in the collection of Hertz’s papers on electromagnetic waves ([2]) he advanced a much more phenomenalist philosophy, and when he returned to the idea of images in his *Mechanics* he had added a requirement of simplicity that made his mature images much less visually appealing. Still his image of mechanics with its hidden, but still visually imaginable, masses is more visual in nature than the usual Newtonian image with its tactile but non-visual forces.

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### **Nicolas Rashevsky and Alfred Lotka: Different modelling strategies in the beginning of mathematical biology in the early 20th century.**

TINNE HOFF KJELDSEN, ANDREA LOETTIGERS

During the 20th century, mathematical modeling has caused changes in scientific practices, and philosophers and historians of science are beginning to investigate the role of models in the production of scientific knowledge. We are concerned with identification of driving forces in the emergence, development and establishment of mathematical biology as a new interdisciplinary research field in the 20th century, and with the epistemic role and status of mathematical models and modelling in mathematical biology. We employ an integrated history and philosophy of science approach. Some of the issues, we are exploring are the development of mathematical practices of model constructions, the identification of various modelling strategies and the explanatory powers of the resulting models. We address questions such as how do mathematicians and scientists argue with and for mathematical models, how and what do they learn, how do they integrate knowledge

across disciplinary boundaries, and what are the problems and benefits of various approaches and strategies.

In the present paper we focus on two of the pioneers in mathematical biology, Nicolas Rashevsky (1899–1972) and Alfred Lotka (1880–1949). We compare and discuss their modelling strategies in some of their early work on cell division and systems approach to biology, respectively.

Nicolas Rashevsky established one of the first groups in mathematical biology and founded *The Bulletin of Mathematical Biophysics* in 1939. Today this is the official journal (renamed *Bulletin of Mathematical Biology* in 1973) for the Society of Mathematical Biology.<sup>1</sup>

Rashevsky was trained as a physicist, and came to the problem of cell division by drawing an analogy to the theory of spontaneous division of droplets. He was searching for the fundamental causes behind biological phenomena, and his ambition was to develop a physicomathematical foundations of biology similar to that of mathematical physics. In 1934 he presented his investigations and results to biologists at a Cold Spring Harbor Symposium on quantitative biology. Rashevsky explained his methodology and his basic scientific presumption in the introduction: "Unless we postulate some factors unknown to the inorganic physical world [...], it is simply a logical necessity, free of any hypothesis, that some physical force or forces must be active within the cell to produce a division of the latter into two or more smaller cells. [...] If however we entertain the hope of finding a consistent explanation of biological phenomena in terms of physics and chemistry, this explanation must of necessity be of such a nature as the explanation of the various physical phenomena. It must follow logically and mathematically from a set of well defined general principles." [6, 188].

Since metabolism occurs in all cells, Rashevsky set out to investigate whether this phenomenon could explain cell division. He drew an analogy to a physical liquid system (like a cell) that "takes in some substance from the surrounding medium, in which this substance is dissolved. If inside the system this substance is transformed into other ones, due to any kind of physico-chemical reactions, there will be a difference in concentration outside and inside the system, the concentration outside being greater. [...] We have to do with a phenomenon of diffusion" [6, 189]. He set up the equation of diffusion for a quasi-stationary state

$$D\Delta^2 c = q(x, y, z)$$

where  $D$  denotes the coefficient of diffusion,  $c$  the concentration, and  $q(x, y, z)$  the rate of consumption of the substance.

Rashevsky made a physical mechanical analysis of the forces produced in a cell/liquid system due to a gradient of concentration produced by metabolism. He derived equations for the various forces, e.g. the force acting on each element of volume due to osmotic pressure. He also derived an expression of the force of attraction between the molecules of the solvent and the molecules of the solute.

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<sup>1</sup>Despite Rashevsky's importance for the establishment of mathematical biology not much has been written about him and his work, see ([1], [2], and [3]).

To estimate the forces on each element of volume of the solvent, he considered the force exerted on a molecule  $A$  of the solvent by all molecules  $B$  of the solute, letting the force of attraction between a molecule  $A$  and a molecule  $B$  be given by  $K/r^n$ ,  $K$  being a constant and  $r$  being the distance between the two molecules. He introduced a coordinate system and chose the axes such that the concentration only varies along the  $x$ -axis. He estimated the forces exerted on the molecule  $A$  by all the molecules  $B$  lying between the two vertical planes through  $x$  and  $x+dx$  and bounded by the two cylindrical surfaces with radius  $r'$  and  $r'+dr'$  and length  $dx$ . By integration, approximations, and estimation of the constant  $K$ , Rashevsky reached an expression for the forces of attraction. He included forces of repulsion, and by adding the three forces he derived an expression for the force per unit volume produced by a gradient of concentration (due to metabolism).

Rashevsky applied the result of his analysis on the simplest case of spherical, homogenous cells. He calculated the free energy in such an idealized system and got the result that when the cell reaches a certain size, the division of the cell will cause a decrease of the system's free energy, suggesting a spontaneous division of the cell. He was very much aware that the situation in biological complex systems, even in the case of spherical cells, is not that simple and that a transition from a higher free energy to a lower one does not necessarily happen spontaneously. However, since it is 'quite impossible' that a system will spontaneously change to a state of higher free energy, his investigations established the necessary conditions of spontaneous division. Based on this, he concluded that "every cell, by virtue of the processes of metabolism, [...] contains in itself the necessary conditions for spontaneous division above a certain size." [6, 192] Rashevsky's talk at the symposium was followed by a highly sceptically discussion in which the biologists criticized his approach and, what we today would call his modelling strategy, (see [3]).

Alfred Lotka's name has become well known by giving his name to the Lotka-Volterra model of predator-prey dynamics. It is less well known that this model had been developed and introduced independently by Alfred Lotka and Vito Volterra by making use of very different modelling strategies [4]. In fact the predator-prey model is a special case of a general systems approach, which Lotka had been developing in the 1920's and which he published in a book entitled *Elements of Physical Biology* [5]. In this book, Lotka motivated the development of his approach by putting forward the argument, that in order to gain some understanding about the processes in biological systems, one needs to go beyond the attempt of defining life. Instead he introduced what he called a quantitative definition, which "tells us how to measure the thing defined; or, at the least, one that furnishes a basis for the quantitative treatment of the subject to which it relates." Based on an understanding of living systems as systems evolving in time, Lotka proposed, by referring to the 2. theorem of thermodynamics, that living systems should be approached as systems undergoing irreversible changes. In his systems approach, Lotka furthermore distinguished between micro- and macro mechanics. Micro mechanics focuses on the phenomena of the individual components of the

system and macro mechanics on its *bulk* properties [5, 19]. The analogy to statistical physics and thermodynamics is obvious, but as Lotka explained, is incomplete when it comes to the transfer of concepts. He stated: "So long as we deal with volumes, pressures, temperatures, etc., our thermodynamics serve us well. But the variables in terms of which we find it convenient to define the state of biological (life bearing) systems are other than these. [...] if we seek to make concrete application we find that the systems under consideration are far too complicated to yield fruitfully to thermodynamic reasoning; and such phases of statistical mechanics as relate to aggregation of atoms or molecules, seem no better adapted for the task." [5, 25] Lotka developed his own approach by drawing another analogy namely to physical chemistry. In the framework of this discipline, he treated the evolving system under study as an aggregation of numbered or measured components of specific distinguishable kind. However, this conceptualization does not allow the application of statistical mechanics, which deals with identical particles.

In order to describe the dynamic properties of the system Lotka developed what he called a general kinetic equation. In doing so he started out from the law of mass action used in chemistry to describe the behavior of solutions. In the example he used as an illustration of his procedure, he introduced a system consisting of 4 gram-molecules of hydrogen, 2 gram-molecules of oxygen, and 100 gram-molecules of steam, at one atmosphere pressure, and 1800°C. The equation describing the evolution of the system is of the following form:

$$\frac{1}{V} \frac{dm_1}{dt} = k_1 \frac{m_2^2 m_3}{V^3} - k_2 \frac{m_1^2}{V^2}$$

where  $V$  is the volume,  $m_1$  is the mass of steam,  $m_2$  the mass of the hydrogen, and  $m_3$  the mass of oxygen. The constants  $k_1$  and  $k_2$  are characteristic constants of the reaction such as temperature and pressure. Lotka was not interested in this particular equation but in the more general statement included in the equation according to which "the rate of increase in mass, the velocity of growth of one component, steam (mass  $m_1$ ), is a function of the masses  $m_2$  and  $m_3$ , as well as of the mass  $m_1$  itself, and of the parameters  $V$  (volume) and  $T$  (temperature)"

He then went on by writing down the equation in a more general form:

$$\frac{dX_i}{dt} = F_i(X_1, X_2, \dots, X_n; P, Q)$$

The equation describes the evolution as a process of redistribution of matter among the several components  $X_i$  of the system. Lotka called this equation the 'Fundamental Equation of Kinetics' where the function  $F$  describes the physical interdependence of the several components.  $P$  and  $Q$  are parameters of the system.  $Q$  defines, in the case of biological systems, the characters of the species variable in time and  $P$  the geometrical constraints of the system such as volume, area, and extension in space. The famous Lotka-Volterra equation is 'just' one application of this general approach.

So far our discussion of the two cases shows that there is definitely more than one way of mathematical modelling of biological systems and phenomena. Where Rashevsky in his modelling of cell division followed a bottom up approach Lotka

on the other side made use of a top down strategy in developing his systems approach. It needs to be noticed that Rashevsky had been very much aware of Lotka's work. In the first edition of his book *Mathematical Biophysics: Physicomathematical Foundations of Biology*, he compared his own modelling strategy with the one chosen by Lotka and Volterra.<sup>2</sup> Following Rashevsky, Lotka (and Volterra) dealt: "with the organic world as a whole. [...] they do not go into the consideration of the detailed structure of each individual organism or of the relations to the fundamental parts of this organism to the physical inorganic world" [7, viii]. A second extended edition of his book was published in 1948 and in 1960 a third edition, further extended into two volumes, appeared. In these editions Rashevsky's development of a physicomathematical foundations of biology can be followed and it can be seen that he later attempted to develop general principles.

Besides the differences of their respective approaches, both scientists drew analogies to physics and chemistry in the construction of their models. This strategy provided them with methods, concepts and tools in their modeling efforts but at the same time introduced mathematical as well as conceptual constraints. Following the development of the respective models shows that models are constructed entities, which are open to changes and adjustments. This characteristic makes mathematical models, such as Rashevsky's and Lotka's model, to epistemic resources allowing for an indirect exploration of biological systems and phenomena [8].

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<sup>2</sup>Most probably Volterra would not have agreed to this characterization. (see [4])

## Geometric Models in Mathematics Teaching in Italy at the Turn of the Twentieth Century

LIVIA GIACARDI

Up to the present, the research on geometric models in Italy has been limited to examining and cataloguing the collections existing at various universities [6], but a study on the use of models in university and pre-university teaching does not yet exist. The aim of this talk is to deal with this problem paying particular attention to the period running from mid nineteenth century to the early decades of the Twentieth Century, and focusing on some main points: why Italy remained marginal in the activity of conceiving and constructing geometric models for university teaching; Corrado Segre and the use of models at the University of Turin; models in pre-university schools; the role of models in the “laboratory school”; the new interest in models in the 1950s.

1. As it is well known, the mass production of models began in the second half of the Nineteenth Century mainly in Munich when Felix Klein came to teach at that university and his collaboration with Alexander Brill started [7]. Various exhibitions and the publication of catalogues favoured the spreading of the use of models in teaching at international level in the years to come.

Italy remained marginal in the activity of conceiving and making geometric models, in spite of the many young mathematicians who went to Germany for post-graduate study under the direction of Klein. We can only mention the attempt in 1883, by Giuseppe Veronese, to set up a national laboratory for the production of models, but in spite of the support of important mathematicians the initiative was unsuccessful. Starting in the 1880s, the principal Italian universities generally preferred to acquire models from abroad, mainly in Germany and the first to acquire collections were those of Pisa, Rome, Turin, Pavia and Naples, but in the present state of research, only in Naples there are documented initiatives for designing and producing models (Alfonso del Re, 1901-1906). However there was not a mass production, even if in the same period other mathematicians of this University shared the interest in models, such as Ernesto Pascal and Roberto Marcolongo.

In the Italian panorama, the cardboard model of a pseudospherical surface built in 1869 by Eugenio Beltrami represents an exception (see the paper by R. Tazzioli). From the dialogue between Beltrami and Luigi Cremona – the most influential mathematician in that period in Italy – we can infer that Cremona was certainly interested in models, but not in their concrete construction, as confirmed also by his correspondence with T. Hirst, J. Plücker and Klein, or by the cardboard models that Brill sent as gifts to him.

The peripheral position of Italy in the activity of designing and constructing geometric models for University teaching and research seems to be connected to the fact that models were mainly used for educational purposes and the most important models had already been constructed in Germany. Furthermore, at that time in Italy three different approaches to geometric research could be found: the analytical approach that was more theoretical and abstract (U. Dini, L. Bianchi, etc.),

the study of the foundations of geometry with an emphasis on logical rigour (G. Peano's School), and finally, the working method of the Italian School of algebraic geometry, which attributed great importance to intuition and visualisation, but preferred to make use of "abstract models". Many evidences of this attitude can be found. Besides the famous passage where G. Castelnuovo describes the working method that he and F. Enriques used in their research ([2], 194, see the paper by N. Schappacher), we can quote the words of Enriques, who speaks about "thousand spiritual eyes to contemplate many different transformations [of the figure]; while the unity of the object shines in our mind so enriched, that it allows us to pass easily from one form to another" ([3], 140). In the university textbooks at the turn of the twentieth century (Bertini, Bianchi, Enriques, Severi Castelnuovo, etc.) the representation of geometric models is either not introduced or it is a simple schematic representation. The only treatise found up to the present in which drawings of surfaces appear to recall actual models is *Lezioni di geometria intrinseca* (1896) by Ernesto Cesaro.

2. In any case, collections of models were bought for educational purposes by all the most important Italian universities (see [6]). In Turin the first acquisitions date back to the 1880s, but it was Corrado Segre, the leader of the Italian School of algebraic geometry, who increased the collection of models. In fact he believed that the models could sometimes smooth the path to discovery, making it possible to "see certain properties that with deductive reasoning alone cannot be obtained" ([8], 54). References to models can be found not only in his notebooks relating to the courses of higher geometry (see *Applicazioni degli integrali Abeliani alla Geometria* (1903-04), p. 26, and *Superficie del 3° ordine e curve piane del 4° ordine* (1909-10), p. 176, but also in the notebook concerning the lessons for the future teachers, in <http://www.corradosegre.unito.it/quaderni.php>).

3. If we also consider the pre-university teaching in Italy, the use of models had already appeared in the first half of the eighteenth century in secondary schools and in the training of primary school teachers, in connection with the pedagogical movement promoted in Torino by the educators F. Aporti, V. Troya and A. Rayneri, who maintained the importance of the Socratic and intuitive methods, especially in primary teaching, and the usefulness of manipulating concrete objects (see the book of Rayneri for primary teacher training, *Lezioni di nomenclatura geometrica* (1952) at p. XXXVI). Under Rayneri's supervision, two collections of geometric solids (with 27 and 35 models) were constructed. They were sold by Paravia, a publisher and bookseller in Torino, which at the time had begun to market educational aids for various kinds of schools. In the same period the collection of crystallographic models conceived by Quintino Sella, one of the founders of mathematical crystallography, was particularly widespread. It was used at the Istituto Tecnico and later at the Engineering School in Turin. The use of models for the teaching of geometry was prescribed by the school legislation (see for example the Regulations for primary teacher training schools of 1853, 1867, 1892, 1895, and the mathematics programs of 1881, 1885 and 1890 for technical schools). In the years that followed other publishing houses (such as G. Agnelli in Milan) also began to

publish catalogues of school materials, including collections of geometric models, and to send them to all Italian schools. In the Paravia catalogues increasingly varied and more beautiful collections of geometric models appeared up until the 1960s. In the course of the twentieth century, new publishing houses (such as A. Vallardi in Milan and A. Mondadori in Verona) introduced collections of geometric models in their catalogues and publicized them in the schools. In connection with this activity, the pedagogical congresses and the national exhibitions often featured displays of models for secondary schools, as part of the section for education: Pedagogical Congress (Turin 1869); Italian Industrial Exposition (Milan 1881); Italian Exposition (Turin 1884); National Exposition (Palermo 1891); etc. The representation of geometric models and their nets was also present in several geometry textbooks for the lower level of secondary schools, such as *Nozioni di geometria intuitiva* (1908) by Veronese, but very often there was no mention of the actual construction of models. Furthermore also specific books concerning models were published by R. Barberis, C. Ottini and A. Rivelli, and the book by G. and W. Young *A First Book of Geometry* (1905) was translated.

4. An active use of models in secondary schools is connected with the introduction of the laboratory method in teaching mathematics in the late nineteenth century. The first to propose such an approach was the English mathematician J. Perry at the end of the nineteenth century. In France, after the reform of the secondary school in 1902 E. Borel, together with J. Tannery, created the *Laboratoire d'enseignement mathématique* at the Ecole Normale Supérieure, aimed at training future teachers. Here models were conceived and built for teaching geometry and mechanics. Mention of the use of models and of geometrical configurations as dynamic objects in secondary teaching are found in the *Meraner Lehrplan* (1905), which was inspired by Klein, whose interest in instruments and models as *anschaulungsmittel* in research and teaching of mathematics is well known and studied [7]. In Italy, it was Giovanni Vailati, a member of the Peano School, to propose (1905-1909) an approach to the teaching of mathematics that he called 'school as laboratory'. In particular, he was convinced that the teaching of geometry should be experimental and active because the use of squared paper, drawing and geometric models fosters the development not only of the students' skills of observation, but also those that come into play in the construction of the figures and when comparing them and their parts, by means of measures, decompositions and movements. Various factors prevented the mathematics laboratory proposed by Vailati from becoming widespread in practice not least the fact that not all the mathematicians in Italy shared his methodological approach [4]. The only one who took up the idea of an effective laboratory-type teaching of mathematics was Marcolongo, who, during the national congress of the Italian association of mathematics teachers (Naples, October 1921) set up an exhibition of models and instruments useful for teaching, and gave a lecture on the educational materials [5]. In fact, according to him, "in knowing hands, the small model can be a starting point for observation and for the experimental discovery of new properties that the student will then attempt to prove by rigorous means" (p. 9). At the end of his talk he did not



hesitate to criticize Italian mathematicians who too often disdain practical and experimental aspects and observed that to reverse this trend it was necessary to start from the secondary school.

5. Models acquired new relevance both in secondary and university teaching in the fifties. In 1958 the book *Le matériel pour l'enseignement des mathématiques* was published by the CIEAM, a commission that marked an important turning point in the history of mathematics teaching. In this book, which was translated into Italian, an article by Emma Castelnuovo, a young teacher, daughter of Guido, concerned the use of material objects in the teaching of intuitive geometry, and a large chapter by Luigi Campedelli, professor at the University of Florence, was devoted to the geometric models. Since the 1940s Emma had introduced and experimented a new way of teaching intuitive geometry, that she called *constructive* to distinguish it from the *descriptive* one generally in use up to that time. She affirmed: “We want to emphasize that in any case, the material must be moveable: mobility is what in fact attracts the attention of the child, and that leads from concrete to abstract notions; because the subject of his attention is not the material itself but rather the transformation of the material, an operation that, being independent from the material itself, is abstract” (p. 58). Emma anticipated some of the lines of research concerning the laboratory of mathematics that have characterized research in education in recent decades.

On the other side, after WW II, in Italy there was a revival of interest in university collections of models, due to the fact that most of them had been destroyed during bombings. In 1951 the Italian Mathematical Union promoted the reconstruction of these collections and Campedelli was in charge of this task. The Mathematics Institute of Pavia made its models available and some artisans in Florence reproduced them. Various Italian universities acquired the collections, and models appeared in several university textbooks including those by Campedelli himself.

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## “Clebsch took notice of me”: Olaus Henrici and surface models

JUNE BARROW-GREEN

The (Danish born) German mathematician Olaus Henrici (1840–1919), having spent a short time as an apprentice engineer, began his mathematical studies in 1859 in Karlsruhe where he came under the influence of Clebsch, as he later recalled:

“Of greater importance to me was the fact that Clebsch took notice of me. He induced me to devote myself exclusively to Mathematics. During the three months summer vacation in 1860 I remained in Karlsruhe earning a little money by private teaching. I was honoured by seeing much of Clebsch. Practically every morning I called for him at 10 o’clock for a long walk during which much Mathematics was talked. It was only later that I realised how very much I had learned during these lessons without paper or blackboard.” [1, p.71]

With recommendations from Clebsch, Henrici went to Heidelberg to study with Hesse and in 1863 he took a PhD in algebraic geometry before moving to Berlin to attend the lectures of Weierstrass and Kronecker. Unable to make a living in Germany, he moved to London in 1865 to work with a friend on some engineering problems. The enterprise was not successful so he turned to mathematics tutoring and continued with his mathematical research. Through Hesse he obtained an introduction to Sylvester, and through Sylvester he got to know Cayley, Hirst and Clifford. In 1870 he succeeded Hirst as the Professor of Pure Mathematics at University College, and in 1880, on the death of Clifford, he took over the chair of Applied Mathematics. Four years later, he was appointed as the founding professor of Mathematics and Mechanics at the newly formed Central Technical College where he established a Laboratory of Mechanics, a position he retained until he retired in 1911.

A proponent of pure (projective) geometry and a leading figure in the British movement against the teaching of Euclid (his textbook [2] was satirized by Charles Dodgson [3, pp.71–96]), Henrici produced a number of models of geometrical surfaces, several of which he exhibited in front of the London Mathematical Society (LMS). He promoted the use of models in teaching, encouraging students to construct geometrical models for themselves [4]. (An evocative description of the student workshop at UCL in 1878 is given in [7].) He played an active part in the great exhibitions in London in 1876 [5] and Munich in 1893 [6]—he was part of a three-man British committee for the latter (the others were Greenhill and Kelvin)—and his models feature prominently in both.

Henrici’s “Professorial Dissertation for 1871-72” was entitled “On the Construction of Cardboard Models of Surfaces of the second Order” [8, p.161], and he gave some of these cardboard models to Clebsch (who was by then in Göttingen). It was one of these models—constructed from semi-circular sections—that in 1874 inspired Clebsch’s student Alexander Brill to make similar models of his own ([8,

p.159]) which he exhibited in 1876 with acknowledgement to Henrici. (These models would provide the starting point of the famous Brill mathematical model business which was run by Alexander's brother, Ludwig.)

Three of the most important of Henrici's surface models were those of the third order surface  $xyz = (\frac{3}{7})^3(x + y + z - 1)^3$ , the moveable hyperboloid of one sheet, and Sylvester's 'amphigenous' surface<sup>1</sup>. The first of these, in which the 27 lines (all real) form three groups of nine coincident lines, was initially constructed in cardboard by Henrici who showed it at the LMS in 1869. A plaster model lent by Henrici was displayed at the Science Museum in London where it later became a source of inspiration for the artist E. A. Wadsworth who used it in his 1936 poster advertising the South Kensington Museums.

The moveable hyperboloid of one sheet originated in 1873 as a problem set by Henrici for one of his students. Henrici had expected the construction he had defined to be rigid and was surprised when it was not the case. It turned out not to be difficult to understand why the surface was moveable, and Henrici was led to establish the theorem: "If the two sets of generators of a hyperboloid be connected by articulated joints wherever they meet, then the system remains moveable, the hyperboloid changing its shape" [9]. The properties of the surface became more widely known through a Cambridge Tripos question set by Greenhill in 1878, the solution of which was published by Cayley [10]. Since then the surface has been shown to have applications in connection with the motion of a gyrotory rigid body, and it is still relevant in research today [11].

Of all Henrici's surface models, the most ambitious was undoubtedly the model of Sylvester's amphigenous surface. This 9th order surface emerged out of Sylvester's great paper proving Newton's rule for the discovery of the imaginary roots of a polynomial which Sylvester had published in 1864 [12]. After a long and convoluted algebraic argument in which he had derived the equation of the surface, Sylvester had shown that when a particular plane touches the surface along a particular curve, it divides each half of the space separated by the surface into three distinct parts. And, as Henrici observed, it is this property which connects the surface in a remarkable manner with theory of binary quintics and by which Sylvester had shown how to decide whether the roots of a fifth degree equation are real or imaginary [13], [6, p.173–175]. In March 1865 Sylvester discussed the possible construction of the surface with Hirst and a mechanician at the Royal Society but shortly afterwards told Hirst that he "had thought a good bit upon this wonderful surface since last seeing you . . . [its] form . . . seems to be gradually growing up in my mind but it requires a prodigious effort beyond my present powers of conception to realise it in its totality" [15, pp.184–185]. There is no record of a model of the surface having been made at this time and it seems that one was not produced until December 1870, when Henrici "exhibited a large model of Dr Sylvester's amphigenous surface" [13] in front of the LMS. Since Sylvester had found a much simpler proof of Newton's rule—one which did not involve the amphigenous surface—in the summer of 1865 [14], it is likely that he then lost

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<sup>1</sup>'Amphigenous' is a botanical term which means growing all round a central point.

interest in trying to construct the surface, his interest being rekindled only when he met Henrici. A model of the surface was exhibited by Henrici in 1876, where it was singled out for comment by H. J. S. Smith [5, p.52], and again in 1893, but it appears not to have survived.

Henrici's work on these models all contributed to his growing reputation amongst British mathematicians, and in 1874 it formed part of the citation for his election to the Royal Society. Further, it is notable that a modelling club was established in Cambridge by Cayley and others (including Maxwell as "the custodian of the models") [16, 331] in the aftermath of the British Association for the Advancement of Science annual meeting in Bradford in 1873, the first such meeting attended by Henrici. Geometry had occupied a prominent position at the meeting, and Klein too was among the attendees. The club took an active part in the 1876 exhibition, although it seems to have faded soon after. Sylvester maintained his interest in models and in Oxford in 1887, four years after his return from the United States, he put on a course entitled "Lectures on Surfaces, illustrated by plaster, string and cardboard models" [17, p.229], although it did not draw much of an audience, presumably due to the fact that the subject was not part of the students' examination requirements.

In 19th century Britain Henrici was one of the leading proponents for surface models and he did much to stimulate an interest in them, both in his students and in his peers. His German origin and education, particularly his tutelage under Clebsch, enabled him to act as a bridge between British and German mathematicians interested in models. It is no coincidence that Britain was the largest foreign contributor to the Munich exhibition.

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### The fourth dimension: models, analogies, and so on

KLAUS VOLKERT

The way to the geometry of a four-dimensional space was not straightforward. In principle, such a geometry was possible after the elaboration of solid geometry in analytic form. But there were still some reservations against such a geometry due to the fact that geometry was understood as the study of space and space was considered as three-dimensional. In Moebius' *Barycentrischer Calcul* (1827), we find several instances where he assures his reader that four-dimensional space could not exist and also H. Grassmann stated in the introduction to his *Lineale Ausdehnungslehre* (1844) that his new science is not bound by any restriction concerning dimensions whereas geometry could not go further than dimension three. Around 1850, we find several cautious attempts to transcend this restriction (Cauchy, Cayley, ...) in speaking of pseudo-points and things like that<sup>1</sup>. An important step forward was taken by C. Jordan in his long paper *Essai sur la geometrie a n dimensions* (1875 - a short overview of its content was published before in 1872) in which Jordan developed the geometry of linear (sub-)spaces of an n-dimensional space. But this could still be criticized as being algebra in geometric disguise. Note that neither Jordan nor someone else before tried to give an intuitive picture of a geometric object in the four-dimensional space at all.

Around 1880, the problem of determining the number of regular polytopes in four-dimensional space became rather popular. This is the analog of Euclid's result on today so-called Platonic solids (book XIII, theorem 18a); it was clear that this is a genuine geometric question. In order to arrive at its solution, it is definitely important to have an insight into the structure of those hyper-solids. A rather complete and convincing purely synthetic solution was given by William Irving Stringham in his dissertation (1879) under the supervision of J. J. Sylvester (then at John Hopkins in Baltimore). After having received his degree, Stringham went to Germany to stay with F. Klein in Leipzig, where he gave a talk on his result in Klein's seminar - once again with a lot of picture. After receiving a call from Berkeley, he returned to the States in the same year. Stringham demonstrated that in four dimensions there are six regular polytopes. We cite them here as the hyper-simplex, the hyper-cube, the hyper-octahedron, the 24-cell, the 120-cell and the 600-cell (Stringham had a somewhat awkward terminology of his own, which

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<sup>1</sup>A collection of interesting texts can be found in Smith 1959, 524-545.

disappeared soon). In higher dimensions, there are only three regular polytopes: the analogs of the first three just cited.

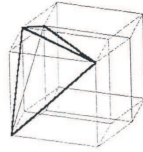


FIGURE 1. The vertex figure of a hyper-cube

FIGURE 1 shows the hyper-cube and its frame, a tetrahedron. The vertex of the hyper-cube is formed by putting a vertex of a cube with the appropriate length on the faces of the tetrahedron. Then, you have to identify the faces of the cubical vertices in order to get one single vertex - that of the hyper-cube. The number of edges meeting in the vertex put on the faces of the frame must be equal to the number of vertices of the faces of the frame. So, Stringham arrives at a list with eleven possibilities (Stringham 1880, 7):

(i.)	4 tetrahedra	upon a tetrahedral frame.
(ii.)	4 hexahedra	“ “ “ “
(iii.)	4 dodekahedra	“ “ “ “
(iv.)	8 tetrahedra	upon an octahedral frame.
(v.)	8 hexahedra	“ “ “ “
(vi.)	8 dodekahedra	“ “ “ “
(vii.)	20 tetrahedra	“ ikosahedral “
(viii.)	20 hexahedra	“ “ “ “
(ix.)	20 dodekahedra	“ “ “ “
(x.)	6 octahedra	upon a hexahedral “
(xi.)	12 ikosahedra	“ dodekahedral “

FIGURE 2. Stringham's list of possible cases.

We can not depict here the quite esthetic figures contained in Stringham's paper (some of them are even in colour). Stringham was obviously a gifted drawer. Please note that Stringham's illustrations are not generated by definite rules; he is most often drawing a sort of perspective picture. In particular, it seems not reasonable to construct material models on their base because this does not provide new information. A systematic way of finding drawings of the regular polytopes was discovered by V. Schlegel in 1884 using the idea of central projection (cf. Schlegel 1884 and Schlegel 1886).

Another type of models of the regular polytopes is obtained by unfolding them. This gives you a three-dimensional object (see FIGURE 3).

This object is sometimes called the tesseract; it was used by S. Dali in painting a crucifixion. The letters indicate the identifications that must be performed to get the polytope.

Howard Hinton, sometimes called the philosopher of the fourth dimension, developed a gadget designed for learning to "see" the fourth dimension. This was composed by a set of colored cubes of equal size. The cubes had artificial names in order to facilitate memorizing the combinations. Hinton was a friend of the

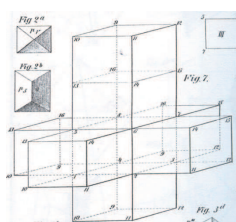


FIGURE 3. The tesseract (Brueckner 1891, plate 1).

family of G. Boole, who died early leaving his wife with five daughters. Perhaps Hinton brought his gadget with him when he visited the family. So - perhaps - one of the daughters, Alicia, played with it and developed in such a way her famous intuition which helped her later to do four-dimensional geometry (e.g. she gave an independent proof of Stringham's result) and to collaborate with the distinguished geometers H. Schoute and H. Coxeter (cf. Polo-Blanco 2007 or Polo-Blanco 2008). Hinton was so much involved in the fourth dimension that he did not notice that he married twice (cf. Rowe 2004); he was accused of bigamy in Great Britain. Hinton fled from there to Japan and later to the States where he worked in the Patent Office at Princeton. Hinton praised the fourth dimension as a spot providing human beings a lot of freedom (at least in thinking and imagination)<sup>2</sup>.

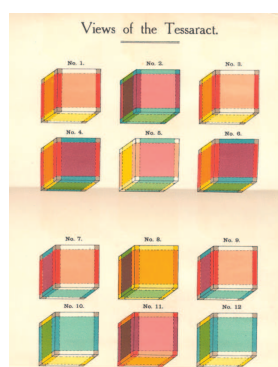


FIGURE 4. Hinton's gadget (Hinton 1921, plate)

For the sake of completeness, I mention here two other ways of presenting four-dimensional objects: one is that of descriptive geometry using two plane ((x,y) and (z,t)) in which the object is projected orthogonally by parallels, the other is that of using a three-dimensional image of the object (in two-dimensional projection) indicating the position of the non-depicted vertices by a segment (in German called *kotierte Projektion*).

For further information on the subject of this summary cf. Volkert 2016.

<sup>2</sup>Around 1880, F. K. Zoellner, a well-established astrophysicist at Leipzig, had provoked a big scandal by using the fourth dimension as an explanation of spiritualistic performances; cf. Volkert 2015 section 5. Henceforth, mathematicians were careful to assure that they do not believe in the real existence of the fourth dimension

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## Around the History of the 27 Lines upon Cubic Surfaces: Uses and Non-uses of Models

FRANÇOIS LÊ

In 1849, Arthur Cayley and George Salmon proved a theorem which can be phrased in modern terms as follows: “Every non-singular cubic surface of  $\mathbf{P}_3(\mathbf{C})$  contains exactly 27 lines.” In the corresponding papers [1, 9], Cayley and Salmon also showed that the 27 lines of a cubic surface are coplanar by threes, thus forming 45 triangles. Later, in 1858, Ludwig Schläfli defined the “double-sixes” of a cubic surface, which are sets of 12 lines (among the 27) with prescribed incidence relations,<sup>1</sup> and he proved that there are exactly 36 double-sixes [10].

The issue of constructing models of cubic surfaces, of the 27 lines without the surface to which they belong, or of a double-six, has been tackled since the beginning of the 1860s. In my talk, I mainly focused on the models of the 27 lines and of a double-six, thus exploring texts of James Joseph Sylvester (1861), Cayley (1870), Percival Frost (1882), and Henry Martyn Taylor (1900).<sup>2</sup> I also aimed attention at two models (including one of the so-called “diagonal” cubic surface) presented in 1872 by Alfred Clebsch. Taking a look at a paper of Clebsch linked

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<sup>1</sup>To be more precise, the lines  $a_1, \dots, a_6, b_1, \dots, b_6$  form a double-six if, in the table

$$\begin{pmatrix} a_1 & a_2 & a_3 & a_4 & a_5 & a_6 \\ b_1 & b_2 & b_3 & b_4 & b_5 & b_6 \end{pmatrix},$$

two lines intersect if and only if they do not belong to the same row and the same column.

<sup>2</sup>The corresponding references have been gathered from the “historical summary” of [5].



to these two models, I finally addressed the question of the actual uses of models in the second half of the 19th century.

### 1. MODELS OF THE 27 LINES AND OF A DOUBLE-SIX

The first considered text is a note of Sylvester to the *Comptes rendus des séances de l'Académie des sciences* of 1861, [11]. In this paper, Sylvester touched upon the idea of creating a model of the 27 lines:

Je me propose de faire construire en fil de fer ou d'archal un système de 27 droites [...], de sorte qu'on pourra éprouver le plaisir inattendu de voir avec les yeux du corps toutes les droites (le squelette pour ainsi dire) d'une surface du 3<sup>e</sup> degré avec leurs 135 points d'intersection, les 45 triangles, [...] et les autres merveilles de cette involution si compliquée, mais en même temps si symétrique. [11, 979-980]

This intention of building a model does not seem to have been realized by Sylvester. However, the quotation shows the latter's interest for the incidence relations linking the 27 lines, incarnated in the 45 triangles for instance. It also hints at the importance of the "skeleton" made of the 27 lines as the key to understand a cubic surface, an idea that has then been used by other mathematicians.<sup>3</sup>

The 1870 article of Cayley deals with a model of a double-six, [2]. In this paper, Cayley started computing equations of the lines of a double-six, as well as their "Plücker coordinates." He then turned to "the numerical computations for enabling the creation of a drawing or model" of the double-six, [2, 68], and calculated the coordinates of the points of intersection of the lines forming the double-six. He concluded: "I find however, on laying down the figure, that the lines 3 and 4, 3' and 4' come so close together, that the figure cannot be obtained with any accuracy." [2, 71]. Cayley did not comment about the creation of a model of a double-six, but the case illustrates the difficulties of finding "good" equations and "good" numerical values when trying to make a satisfying model.

In Frost's paper [4], the emphasis was also put on the issue of finding simple equations for the lines as well as adequate numerical values, so that the points of intersection of the 27 lines would not appear too close. Frost encouraged his readers "to spend a few minutes on the subject, and possibly to amuse themselves, as [he has] done, by constructing a model" [4, 89], which points to a recreational aspect of the building of a model. However, he admitted to have failed creating a complete model:

I shall be most happy to show what I have done to anybody who may like to see what to avoid and what to adopt. My model is anything but perfect, two or three of the lines are too far off to appear, and with them their ten points of intersection are out of sight. The only satisfaction I have is that I know where they all

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<sup>3</sup>For example, when Hieronymous Zeuthen investigated the possible shapes of cubic surfaces in [13], he used the 27 lines to define their "sides," their "triangles", and their "openings".

are, and 9 or 10 lines all pointing at the deserters tell where they ought to be. [4, 96]

This statement hints at the comings and goings of the process of creating a model; it also emphasizes that trying to build a model requires investigations which allow one to gain knowledge of the situation even though the final model is not complete.

Taylor's 1900 article [12] essentially involves the same questions and ingredients as the preceding (finding simple equations and adequate numerical values) but ends up with a success. For the sake of brevity, I only display here two pictures of Taylor's 27 lines model, without commenting any further (see figure 1).

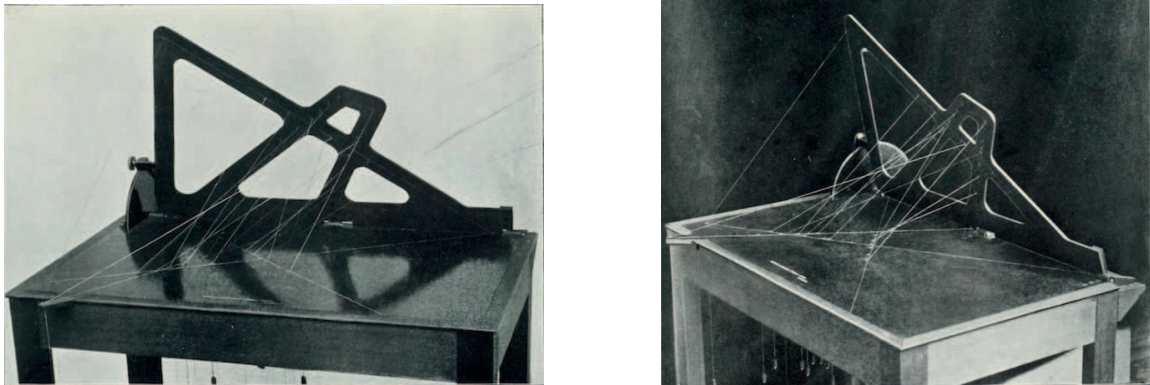


FIGURE 1. Model of the 27 lines made of chord, [12].

## 2. FROM THE DIAGONAL SURFACE TO GEOMETRICAL EQUATIONS

The *Nachrichten der Königlichen Gesellschaft der Wissenschaften und der G.A. Universität zu Göttingen* of 1872 contains an account (p. 402) of when Clebsch presented two models:

Hr. Clebsch legte zwei Modelle vor, welche Hr. stud. Weiler hieselbst dargestellt hatte, und welche auf eine besondere Classe von Flächen dritter Ordnung beziehen. [...] Das eine der beiden Modelle stellte die 27 Geraden dar, das andere die Fläche selbst, ein Gypsmodell, auf welchen die 27 Geraden gezeichnet waren.

The rest of the account does not mention the first model anymore; it describes the second one, with an insistence on the shape of the “diagonal surface.”

This surface had appeared in a 1871 paper of Clebsch where he intended to geometrically interpret the theory of the quintic equation, [3]. One of his aims was to interpret the “Tschirnhaus transformation”  $\xi = a + \lambda b + \lambda^2 c + \lambda^3 d + \lambda^4 e$ , which was supposed to act on a quintic  $f(\lambda) = 0$  so that the transformed equation would have the form  $\xi^5 + A\xi + B = 0$ . The roots of the transformed equation being noted  $\xi_1, \dots, \xi_5$ , the transformation would have the desired effect when

$$\sum_{i=1}^5 \xi_i = 0, \quad \sum_{i=1}^5 \xi_i^2 = 0, \quad \sum_{i=1}^5 \xi_i^3 = 0.$$

These conditions led Clebsch to study the two surfaces defined by  $\sum \xi_i = \sum \xi_i^2 = 0$  and  $\sum \xi_i = \sum \xi_i^3 = 0$  respectively. The second surface is the one that Clebsch called “diagonal surface.” The associated “27 lines equation” and “36 double-sixes equation” then played an important role in Clebsch’s final interpretation of Kronecker’s approach of the quintic.

In fact, these equations are part of a larger family, that of “geometrical equations,” which are algebraic equations (in one unknown) associated to diverse geometrical configurations like the 27 lines or the 36 double-sixes, but also the 9 inflection points of the cubic curves, the 16 nodes of Kummer’s surface, etc. Geometrical equations played a crucial role (ca. 1870) for a geometrical, intuitive understanding of the theory of equations and the theory of substitutions for people like Clebsch and Klein among others:<sup>4</sup>

Der hohe Nutzen dieser Beispiele liegt darin, daß sie die an und für sich so eigenartig abstrakten Vorstellungen der Substitutionstheorie in anschaulicher Weise dem Auge vorführen. [6, 346]

A central feature of this “intuitive” approach consisted in replacing the search of resolvents of a geometrical equation by the search of configurations made from the objects linked to the main equation. For instance, the very existence of the 36 double-sixes meant the existence of a resolvent of degree 36 (the double-sixes equation) of the 27 lines equation.

Now, I found no evidence in favor of any kind of use of models for issues relative to geometrical equations<sup>5</sup>—for instance, one could have expected that the inspection of a model of the 27 lines would have made someone discover a new configuration made from these lines, and therefore a new resolvent of the 27 lines equation. So, even if models were used to find new results in some cases, this was only true for certain situations. As for geometrical equations, even though the involved objects were the same as those which were modeled, and even though the mathematicians were essentially the ones implied in the production of models, these models were not used. Therefore, this observation helps us delimit the realm of uses of models in the mathematical research of the 19th century. Additionally, it proves that the “intuition” mobilized for geometrical equations was not necessarily related to a concrete kind of visualization supported by material objects like models.

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<sup>4</sup>The mathematical activities linked to geometrical equations are analyzed in [7] and [8], where the notion of “cultural system” is discussed as descriptive category of these activities.

<sup>5</sup>Either in the published texts, the letters, or the manuscripts I studied.

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## On Alfred Clebsch and Cubic Surfaces

OLIVER LABS

Alfred Clebsch is known for many reasons. The talk explained some of his not so well-known results which might help to answer some questions related to the history of geometric models of cubic surfaces. The topics mentioned were: The pentahedron related to a cubic surface, its hessian intersecting it in the parabolic curve, its covariant of order nine intersecting it in the cubic’s 27 straight lines, and Clebsch’s representation of cubics as a plane together with 6 points.

This brief article discusses only one aspect in some detail: Why were there essentially no good models of cubic surfaces before the Clebsch diagonal cubic surface model in 1872 whose modelling was performed by A. Weiler, but initiated and presented by A. Clebsch [6]? A similar question was asked by F. Lê in his talk (right before the author’s) on 19th century string models showing the lines (not the surface) of non-ruled cubic surfaces; e.g. he mentioned that several researchers failed, including A. Cayley. One reason may simply be that none of the predecessors had enough experience with equations of cubic surfaces in  $\mathbb{R}^3$  to be able to come up with good equations — despite their deep knowledge on equations of surfaces in projective space (see, e.g. [1]). But using J.J. Sylvester’s pentahedron [13] — five specific planes related to a given cubic surface — A. Clebsch (who proved Sylvester’s conjecture [2]), and in particular later C.F. Rodenberg were able to find not only good equations in projective space, but also in  $\mathbb{R}^3$  without having to perform a very large number of time-consuming calculations (see [12]).

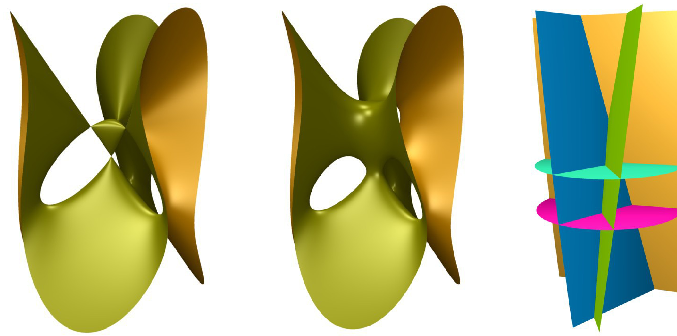


FIGURE 1. Left: The 4-nodal cubic. Middle: The smooth Clebsch Diagonal Cubic. Right: The pentahedron of both.

To illustrate how knowledge on the pentahedron of a cubic surface helps (and actually did help Clebsch, Weiler, see [6], and later Rodenberg) to make good models of cubic surfaces, let us start with the famous diagonal surface which Clebsch studied in [5]. Sylvester's form of equation for a general cubic surface which is based on five planes  $p_1(x, y, z) = 0, \dots, p_5(x, y, z) = 0$  is  $\sum (\frac{1}{a_i})^2 (p_i)^3 = 0$  where  $a_i \in \mathbb{C}$  are coefficients and where  $\sum p_i = 0$ . The diagonal surface is the example for which  $a_i = 1$  for all  $i$ . For Weiler's model the first four planes were chosen to form a steep tetrahedron: three planes with a three-fold rotational symmetry w.r.t. the vertical axis of the coordinate system, and the fourth plane taken horizontally. Via the linear condition on the  $p_i$  the fifth plane is then also a horizontal one such that the whole object has three-fold rotational symmetry (see figure 2). For the first three, one may take (see also [9])

$$\begin{aligned} p_1 &= -\frac{\sqrt{6}}{3}x + \frac{1}{3s}y + \frac{\sqrt{2}}{3}z - 1, \\ p_2 &= \frac{\sqrt{6}}{3}x + \frac{1}{3s}y + \frac{\sqrt{2}}{3}z - 1, \\ p_3 &= \frac{1}{3s}y - 2\frac{\sqrt{2}}{3}z - 1, \end{aligned}$$

then  $p_4 = 3 + \frac{1}{s}y$ , and thus  $p_5 = -(p_1 + p_2 + p_3 + p_4) = 3 - \frac{2}{s}y$ . Here,  $s$  is a factor, e.g.  $s = \frac{5}{2}$ , one may use to scale the surface along the symmetry axis in order to obtain an object with elegant proportions after cutting it by some vertical cylinder. From many modelling experiments, I have the impression that sometimes using the golden ratio is not a bad idea, so the author's most recent series of 45 3D-printed cubic surfaces — permanently exhibited at the university of Strasbourg (France) — incorporates this in many of the models.

Now, as Rodenberg explains in [11] and uses in [12], for a cubic in pentahedral form to have four conical singularities (written  $C_2$  in 19th century notation, locally looking like a quadric cone, also called ordinary double point or  $A_1$  singularity in modern terminology) exactly four of the  $a_i$  should be equal, e.g.  $a_1 = 1, \dots, a_4 = 1$ , and finally  $(\frac{1}{a_5})^2 = \frac{1}{4}$ . Then,

$$\sum (\frac{1}{a_i})^2 (p_i)^3 = 0, \quad \sum p_i = 0,$$

is a cubic surface with four singularities if the  $p_i$  are general enough. Taking the same pentahedron as above for Clebsch's diagonal surface, one obtains a shape which is very similar to that one, but this time with four of the 'tunnels' shrunked to one singular point each (see figure 1).

Using a computer software such as the author's tool SURFEX (from 2001) or the user-friendliness-wise more mature tool SURFER (developed by the MFO in collaboration with the author in 2008) it is straight forward to visualize the deformation of one surface into the other by varying the parameter  $a_5$ . During the talk, some of the main ideas were illustrated using SURFEX, but some also using the author's tool XCSRPG which is specialized on the plane representation of cubics (see also [10]), and also using some of the author's 3D-printed and laser-in-glass models.

The talk finished with a list of related questions which might be worth studying, one being: Why are there so few non-ruled historical surface models only showing the surface instead of a whole body of plaster of which only the surface is the mathematically interesting part (see figure 2)? Some mathematical advantages of the filmy models over the historical plaster models were mentioned in the talk and illustrated with the help of concrete objects, in particular the fact that some of the 'tunnels' which are important for the surface's topology are not visible in the historical plaster models. Back in 1892, it seems that W. Dyck was also puzzled about this when he edited the catalogue for the 1893 exhibition in Munich. In a footnote to a filmy (!) minimal surface model on page 297 which was originally modelled by C. Schilling in Göttingen, but later produced using galvanism at the *Technische Hochschule München* he writes [7]: “*Die Vorteile einer solchen Darstellung, welche den eigentlichen Charakter einer Fläche zur Anschauung bringt, tritt hier gegenüber den stets mehr als Körpern erscheinenden Gipsmodellen und ähnlichen deutlich hervor.*”

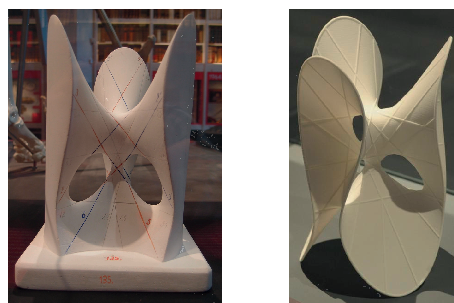


FIGURE 2. Left: Historical plaster model of Clebsch's diagonal cubic with 27 real straight lines (photo: Wikipedia, Oliver Zauzig) where three of the tunnels are filled with plaster. Right: a 3D-printed model by the author (from 2012).

On Thursday, I organized a small recreational session in two parts (after lunch and after dinner) where some of the participants did some paper folding and made some soap water minimal surface experiments using some of the author's models.

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**Visualising the Boy surface**

FRANÇOIS APÉRY

In this talk, models figure in the material sense. It is a quite common opinion that mathematics is a purely mental activity based on a formal language ruled by logic, and hence practical applications and physical objects would be something of a miracle. We don't know why they happen but they do, that is the way it is. An explanation might lie in a logical reversing. Mathematical models are not only teaching tools but can also have inspiring influence. I would like to lay particular stress on the heuristic value of mathematical models as an aid to discovering new truths in mathematics. Since the Boy surface, which is an immersed image of the real projective plane in  $\mathbb{R}^3$ , is an emblematic object of the mathematical institute here in Oberwolfach, my choice was easy. One can touch the model exhibited on the grass of the Institute.

Between Werner Boy thesis defense in 1901 and the construction of the Oberwolfach model of the Boy surface in 1987, visualisation has played a major role in the emergence of more and more mathematically rich models. One can distinguish four aspects in the visualization of the Boy surface: topological, polyhedral, differential and algebraic.

I am going to discuss briefly the first three aspects prior to focusing on the algebraic point of view. The topological study was initiated by Boy himself in trying to construct a counterexample of Hilbert's assertion that it is not possible



to immerse  $\mathbb{P}^2$  into  $\mathbb{R}^3$ . He imagined a surface constructed by a sequence of level curves and proved the expected result by analyzing the critical points of the height function in terms of “Morse theory”, as we call it today. Soon after Boy’s construction Hilbert observes that the existence of a triple point allows one to build a model with an order three symmetry axis.

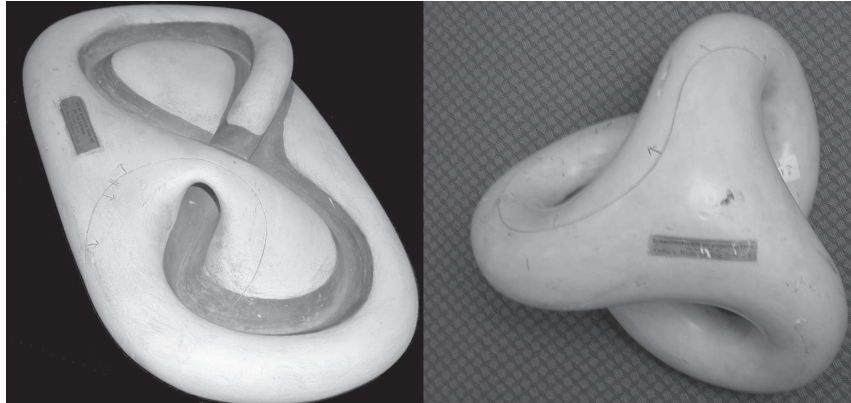


FIGURE 1. (left) Asymmetric Boy’s model. (right). Symmetric Boy’s model (IHP collection).

Regarding the polyhedral point of view one can emphasize two steps. Firstly the cuboid version of the Boy surface (J.-P. Petit 1985). Secondly, the polyhedral Boy surface with nine vertices, which is a minimum (Ulrich Brehm 1990) and comes from the cell complex defined by a polygonal decomposition of the projective plane into 3 pentagons and 7 triangles. Remarkably that it can be immersed into  $\mathbb{R}^3$  by a map affine in restriction to each facet.

The differential approach culminated with the construction of a Willmore Boy surface in 1984 derived from the works of R. Bryant and R. Kusner.

Now I must apologize for focusing on my own research, that is finding an algebraic equation of degree six for the surface. The point is that physical models have been used all along to produce an algebraic object in a constant back and forth between geometric intuition and theory.

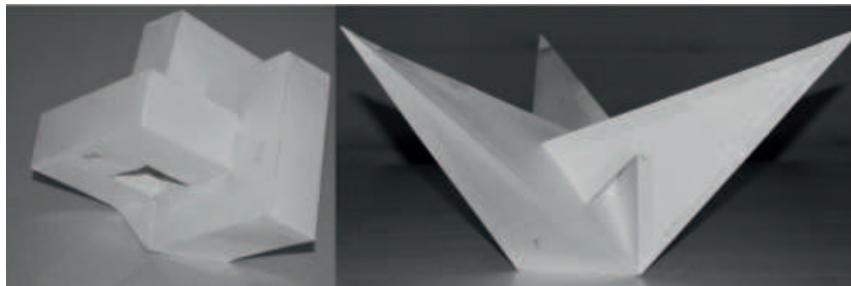


FIGURE 2. (left) Cuboid model of the Boy surface (after J.-P. Petit). (right) Boy polyhedron with nine vertices (after U. Brehm).



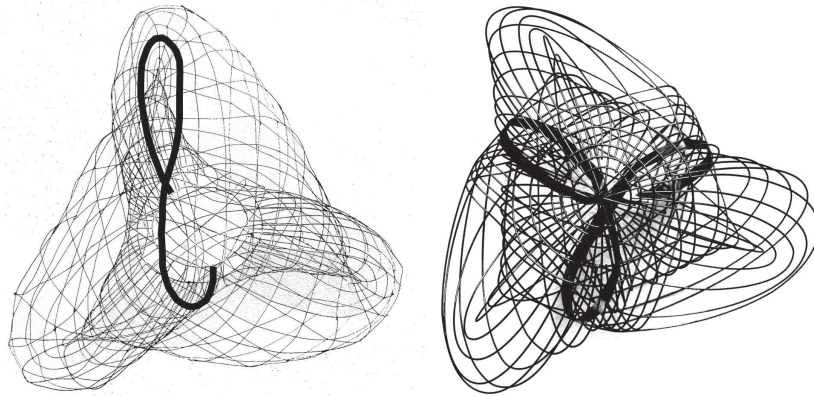


FIGURE 3. (left) Wire model generated by twisted curves. (right). Sauze's model generated by plane oval curves.

The problem has been set by Boy in 1901:

*Die Gleichung dieser Fläche wird mindestens von sechsten Grade.*

In 1932, Hilbert, wrote:

*Ob es algebraische Flächen von der Gestalt der Boyschen Fläche gibt, ist noch nicht untersucht.*

Using models in Plasticine, in 1978 Bernard Morin gave a parametrization of the Boy surface based on two ideas: first, the surface is generated by a family of curves passing through a fixed point, namely the images of a pencil of lines in  $\mathbb{P}^2$ , and second, the apparent view from the center is a closed curve with 3 cusps. Morin constructed a map from  $S^2$  into itself, invariant by the antipodal action on the source, the critical set being a hypocycloid with 3 cusps up to the inverse of a stereographic projection. The final step consists in a perturbation by a radial coefficient in such a way that the map is of rank 2.

Using the same constraints Jean-Pierre Petit realized an empirical wire model which was exhibited for a while at IHES in Bures-sur-Yvette.

Since the model was not aesthetically satisfactory Morin asked the sculptor Max Sauze to build another model. In order to make the job easier, the sculptor suggested replacing the twisted generating curves by plane oval curves. The result is topologically equivalent.

In 1981 J.-P. Petit and J. Souriau gave an empirical parametrization of a Boy surface generated by ellipses passing through the pole. Then Morin noticed that the apparent view from a point located outside the surface on the axis of symmetry is a curve with three cusps and of index 2 about the axis. When the point move on the axis of symmetry toward the pole, the apparent view splits into a simple curve with three cusps and an oval which tends to infinity when the point tends to the pole.

It is known that the Roman Steiner surface is an image of the real projective plane which is generated by ellipses passing through a fixed point located on a threefold axis of symmetry. In addition, the apparent view from this fixed point

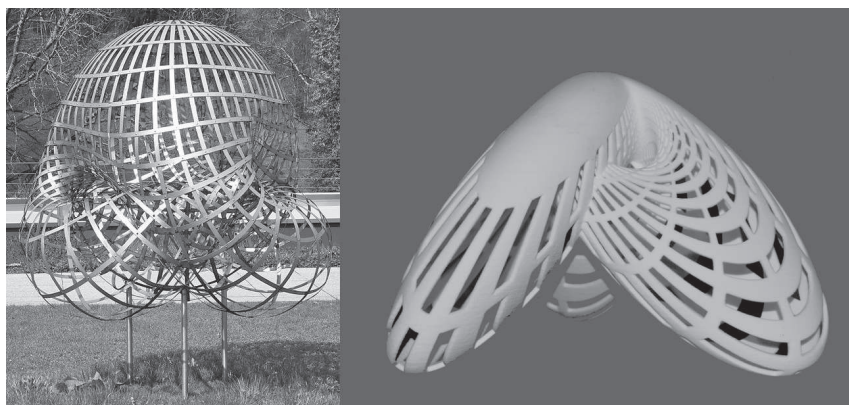


FIGURE 4. (left) Mirror image of the Boy surface (Oberwolfach). (right) Direct Boy surface of degree six (Göttingen Model Collection).

is a hypocycloid with three cusps. However it is not an immersed surface because of the existence of six Whitney umbrellas.

In 1984 I succeeded in eliminating the singularities of the Roman surface by deforming in their planes the ellipses coming from one of the four poles, so that the apparent view from the pole remains unchanged. During the process the Whitney umbrellas are eliminated by pairs using the so-called “hyperbolic confluence”. So doing, we get a polynomial  $P \in \mathbb{Z}[\sqrt{3}][X, Y, Z, t]$  such that

$$\begin{cases} \deg_{X,Y,Z} P = 6, \deg_t P = 3, \\ P(\cdot, \cdot, \cdot, 1) = \text{Boy surface}, \\ P(\cdot, \cdot, \cdot, 0) = \text{Roman surface} + \text{double plane} \end{cases}$$

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## Remarks about Intuition in Italian Algebraic Geometry

NORBERT SCHAPPACHER

Italian Algebraic Geometry has repeatedly been criticised for its alleged lack of rigour. Accordingly, the first rewriting of Algebraic Geometry, which was realized in the 1930ies and 1940ies principally by Bartel L. van der Waerden, Oscar Zariski and André Weil, has been portrayed as a restoration of rigour in this domain. Furthermore, in most accounts of these events the criticism for lack of rigour is linked to the alleged *intuitive* character of the Italian research on Algebraic Geometry – see the three clippings below. The aim of the talk was to show that this association of intuition with lack of rigour – a topos which incidentally is in itself interesting from a historical or philosophical point of view – is misleading and ought to be discarded in order to clear the way for an adequate historiographical appraisal of the Italian production.

Just two quotes to illustrate what I am alluding to: Commenting on his 1941 paper “On the Riemann hypothesis for function fields”, André Weil recalled in 1979 discussions about Algebraic Geometry from the 1930ies and 1940ies (my translation from [11], p. 555):

There was still quite a bit of confusion as to Algebraic Geometry. A growing number of mathematicians, among them the followers of Bourbaki, had convinced themselves of the necessity to ground all of mathematics on set theory; but others had doubts whether this would be possible. As counterexamples they pointed to probability calculus, differential geometry, and algebraic geometry. They claimed that these needed autonomous foundations, or even (thus confusing the needs of invention with those of logic) that they required the constant intervention of some mysterious intuition. But it had become increasingly difficult to sustain an unlimited confidence in Algebraic Geometry. Too many fractures appeared which made one fear that the whole edifice would collapse at the next blow. This is what Zariski experienced when he wrote his famous volume *Algebraic Surfaces* whose explicit goal was above all the critical evaluation of the main discoveries of Italian geometers in their favourite area of research.

And in 2009, we read on the first page of [1] (and we wonder which other ‘schools’ the authors may be thinking of):

There were, of course, other important schools of algebraic geometry in other countries, but the Italian school stood out because of its unique mathematical style, especially its strong appeal to geometric intuition.

Since intuition, taken in a broad sense, accompanies any scientific activity, we have to make our question more precise: To which extent did the geometric visualisation of the researched objects constitute validating elements of mathematical

proof in Italian Algebraic Geometry, say, between the 1880ies and 1920ies? To answer this question we will look at the way in which figures relate to the surrounding discourse in various sorts of texts produced by Italian Algebraic Geometers. Four cases were presented in the talk:

(1) The *Encyklopädie* overseen by Felix Klein. It contains well illustrated chapters, for instance the one on (systems of) conic sections [7], where figures are linked to the text almost as closely as in Euclid's *Elements*. Also Kohn's and Loria's exposition of special algebraic curves [10], which introduces basic objects of Algebraic Geometry, contains at least occasional illustrations. The most spectacular drawings of surfaces can be admired in the chapter on topology [6], see for instance p. 197. All these images show that there were no production constraints on inserting figures into the text in the *Encyklopädie*. Yet the two famous chapters on algebraic surfaces, [3] and [4], by Guido Castelnuovo and Federigo Enriques carry not a single illustration. By the way, the same is true of Enriques's textbook [9].

(2) But to be sure, our Italian Algebraic Geometers *were* in the habit of using illustrations in other sorts of texts they produced. Looking for instance at Enriques's lectures on Projective Geometry [8], we find numerous lettered diagrams which are clearly meant to be read as part of the proofs given. This means that substantial basic knowledge required of any researcher preparing to work in Algebraic Geometry was invested with an essential illustrative component. More generally, there can be no doubt that basic objects of algebraic geometry – such as individual algebraic curves, for example – were naturally pictured (with or without actually drawing them) by all those working with them.

(3) This basic reflex of visualising given objects and constellations is nicely documented in the recently edited notes, by an unidentified notetaker, of Castelnuovo's last lecture course (1922–23) on plane curves and space curves [5]. Studying these notes, one begins to understand why figures tend to disappear from research publications in Algebraic Geometry. In fact, as they were shaping algebraic geometry, the Italian geometers were led to analyzing constellations of objects which are increasingly difficult to visualise adequately: Their work takes place in *iperspazio* – i.e., in higher dimensions, often needed to conveniently project down from, even when the basic objects are initially given in the plane or in 3-space. Furthermore, adjoint objects, linear series, and other families of geometric objects are studied which cut out things on underlying geometric objects. These families are often defined, and always ultimately controlled in terms of polynomial algebra. The corresponding arguments can typically not be drawn, nor can subtle genericity assumptions – another hallmark of classical algebro-geometric reasoning – be visually controlled in a figure.

Thus turning pages in [5], we find many results accompanied by skilful illustrations of the situation addressed. However, these do not visualise the core arguments of the proof which is then developed. In the talk, this was explained for Theorem 3.11 ([5], p. 45, where the adjoint curve whose existence is finally established as an application of polynomial algebra via Noether's theorem is not shown

in the figure), Lemma 3.26 ([5], p. 48, where the existence claim is established by counting constants), and Theorem 4.28 ([5], p. 67–68, where the higher dimensional situation is beautifully sketched, but the proof is quintessentially algebraic in that it rests on the independence of various intersection conditions).

(4) We have also inspected the occasional jottings in Enriques’s letters to Castelnuovo [2] (in the talk we just commented the drawings on p. 244 and p. 477). They confirm the impression that such spontaneous sketches of geometrical constellations are not meant to carry the weight of a geometric construction or a concluding argument.

To sum up, since objects whose existence is finally established in an algebraic way are typically absent from the drawings, it is plausible to interpret the drawings as a spontaneous reflex when setting up an investigation, rather than viewing them as a key element of the argument. Even choices made in the course of a proof – often of objects in general position – are typically not recorded in the diagrams. If lack of rigour there was, it thus has to be studied in the discourse and the algebraic reasoning of the Italian Algebraic Geometers.

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## Algebraic writings as models: the theory of order in the 19th century

FRÉDÉRIC BRECHENMACHER

### INTRODUCTION: ON ALGEBRAIC WRITINGS AS MODELS

To be sure, algebraic writings can hardly be considered as “models,” whether in the sense of concrete geometrical models, or in the sense of the mathematical models used in the natural sciences. Yet, because the contemporary concept of model did not already exist in the 19th century, investigating the history of this concept involves looking at a variety of lines of development which are not limited to geometry or applied mathematics.

Algebraic writings actually share several characteristics with both geometrical and mathematical models. First, even though their materiality is rather made of ink or chalk than plaster or wood, they provide forms of visualization and support manipulations, procedures, and intuitions. Second, before the development of abstract and structural algebra, a number of special equations played a role similar to the one mathematical models would play in the 20th century (e.g. cyclotomic equations, the secular equation, geometrical equations, etc.). These equations were especially used for drawing analogies between domains, uncovering hidden relations or objects, and for transferring methods from one problem to another [1].

The present talk is a case study on the model role played by algebraic writings in a specific 19th century framework: the “theory of order.” This terminology was rarely defined explicitly and was even conceived quite differently by various scientists. But despite its multifaceted nature, the theory of order nevertheless always presented the following characteristics:

- the investigation of the order or the situation of general classes of objects, rather than the magnitude or proportion of specific objects,
- a transversal approach to a great variety of domains in the mathematical sciences: number theory, algebra, geometry, mechanics, crystallography, etc.
- key notions: cyclotomy, congruences, combinations, symmetries, groups, polygons and polyhedrons,
- the interplay between specific forms of visualization, especially the analytic representation of linear groups, the symmetries of polygons and polyhedrons, and the mechanical motions of solid bodies.

### 1. CAMILLE JORDAN’S THEORY OF ORDER

Camille Jordan was an important actor of the theory of order in the 1860s. A reference to the general framework of the theory of order already occurs in Jordan’s very first mathematical work, i.e. the first thesis he defended at the faculty of science in Paris in 1860. This thesis is devoted to the problem of the “number of values of functions,” which is one of the roots of the theory of substitution

groups.<sup>1</sup> Its main result was the introduction of the general linear group  $Gl_n(F_p)$ . Yet, this group was not generated by a list of axioms, as would be natural to mathematicians nowadays. On the contrary, linear substitutions were above all identified by a very specific form of visualization: their analytic, i.e. polynomial, representations, which required to provide an indexation for the letters.

This approach was explicitly presented as a generalization to systems of  $p^n$  letters (i.e. Galois fields) of Gauss' 1801 indexation of the roots of cyclotomic equations  $x^p - 1 = 1$ ,  $p$  prime. The organization of the roots in a specific "order" by appealing to the two indexings provided by a primitive root of unity and by a primitive root mod.  $p$  allowed a decomposition into "groups," as Poincot had designated them in 1808. Moreover, Poincot had discussed this decomposition from a geometrical perspective. The roots generated by a primitive root of unity could be represented "as if they were in a circle" [3]. The substitutions could thus move the indices of the roots forward either by translations, i.e., by the operation represented analytically by  $(x \ x+k)$ , or by rotations of the full circle, i.e.,  $(x \ gx)$  [2]. In Poincot's views, Gauss' two indexations thus established a connection between arithmetics (congruences), algebra (factorization of equations), geometry (circular representations), and mechanics (translations and rotations on/of a circle).

Jordan commented his own procedure of reduction of groups in analogy with the decomposition of a helicoidal motion into motions of translation and rotation which, again, referred to Poincot. According to Jordan the method of ordering the indexes of letters was pointing to a more "essential principle" than the specific problem of the number of values of functions, i.e. "what Poincot has distinguished from the rest of mathematics as the theory of order."

Jordan developed further his investigations on the general linear group throughout the 1860s. Procedures of reductions of the analytic representation of substitutions played a key role in these investigations, which would culminate in the 1870 *Traité des substitutions et des équations algébriques*. The statement of the Jordan canonical form theorem between 1868 and 1870 exemplifies that this specific form of visualization provided a model for generalizations, analogies, and transfers of methods between domains.

In addition to the topics related to Galois, Jordan published memoirs on the symmetries of polyhedra, crystallography, the groups of motions of solid bodies, the analysis situs of the deformation of surfaces, and the groups of monodromy of linear differential equation. In the opening of the notice he wrote in 1881 for his application to the Académie, Jordan insisted that mathematics is not limited to magnitudes or proportions but that it also deals with "order" as well as with "situation." Referring to Poincot, Jordan claimed that besides "ordinary Algebra" one finds the "Algèbre supérieure" that is based on "the theory of order."

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<sup>1</sup>This problem is tantamount to finding the possible orders for subgroups of the symmetric group.

## 2. THE THEORY OF ORDER IN THE 19TH CENTURY

In a word, the theory of order was characterized by Jordan as the part of mathematics dealing with “relations” between general classes of objects. The specific algebraic writings at the core of Jordan’s understanding of the theory of order played a role very similar to the one that modern mathematical models play in the natural science. But, in contrast with the modern concept of mathematical model, the transfers between domains were not based on the identification of abstract objects or structures, but on the analogies revealed by the possibility to use the same algebraic visualizations, and therefore procedures, in various domains.

Identifying the collective dimensions of the theory of order in the 19th century raises difficult issues. On the one hand, this framework was never universally shared by mathematicians. For instance, prominent actors in Paris and Berlin such as Hermite and Leopold Kronecker strongly rejected the generality of Jordan’s approach to classes of objects [5]. On the other hand, various scientists all over Europe were promoting approaches very similar to Jordan’s theory of order by investigating symmetries, combinations, topological situations, or cinematic geometry. Among these actors, most French scientists had been trained at *École polytechnique*, which suggests the development and the transmission of a specific mathematical culture in this institution. But these issues require further investigations on the teaching at polytechnique in the 1850s.

### CONCLUSIONS

The theory of order can be considered as a specific 19th century model of scientificity, among others. For this reason, it raises issues that are not limited to the framework of the mathematical sciences. The 19th century may actually be designated as the century of “order.” The terminology “order,” “theory of order,” or “science of order,” was as much used, and as multifaceted, as the one of “nature” in the 18th century, or the one of “model” in the 20th century. This terminology was especially instrumental in the development of positivism, such as in the approaches developed by the polytechnician Cournot to both mechanics and economy. Order was also considered as an essential concept by philosophers of science such as Louis Couturat, in the legacy of René Descartes, Poincaré, and Galois. It also played a key role in art theory, especially in the theorization of arabic “entrelacs” by Jules Bourgoïn, who had managed to access some of the lectures at polytechnique [4]. The “theory of universal order” also played a key role in moral sciences and esthetics in Victor Cousin’s legacy. We shall also mention the “science of order” developed for public administration as a method of organization and classification.

The historical investigation of the theory of order raises issues similar to the one of the concept of mathematical model. Even if limited to mathematics, both concepts are multifaceted and follow various lines of developments. In the 19th century, a number of mathematicians developed various interpretations of the theory of order. Some referred mainly to Poincaré’s approach to cyclotomy while others rather pointed to André-Marie Ampère’s molecular systems in chemistry,



to Cournot's mechanics, or to Bravais' crystallography. Moreover, some made extensive uses of geometrical visualization, such as Catalan and Bertrand, or even of models of crystals made of woods, such as Bravais, while some others, such as Jordan, never displayed any geometrical figure but appealed to algebraic visualizations. In the framework of the theory of order, it is therefore not relevant to oppose the forms of visualizations provided by algebra and geometry.

As for the concept of model, the theory of order points to a complex history, with a variety of actors who shared an interest for general classifications, ideals of uncovering hidden relations between domains, and key notions such as combinations, symmetry, situation, polyhedrons. Yet, their works show no direct continuity but rather different lines of development in connection with contemporary evolutions in other fields than mathematics, such as philosophy, economy, politics, etc. A multi-disciplinary research group has recently been organized by Jenny Boucard (University of Nantes) and Christophe Eckes (University of Lorraine) for investigating further the multi-faceted history of the theory of order.

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### Visual reasoning in early electromagnetism

FRIEDRICH STEINLE

My talk deals with the question of how experimentalists have used visualizations to make sense of their experimental results or, more specifically, to be able to bring their findings under general rules. I focus on the question of what role visualizations played in those processes, and in what way they contributed to the result that researchers finally drew and formulated. I shall treat those questions not in general, but by analysis of a particularly interesting historical case, viz. early electromagnetism.

Electromagnetism, since its discovery in 1820 by the Danish physics professor Hans Christian Oersted, was confronted with the conceptual problem of correlating spatial directions. Oersteds experimental arrangement consisted of a magnetic needle, suspended like a compass, and a “connecting wire” of a galvanic battery (a current carrying wire in our terms) held horizontally above the needle. When the wire was connected to the battery, the needle was deflected: it turned out of its normal north-south direction. However, to describe how exactly that deflection

took place, in which direction and depending on which other parameters (such as relative position of wire and needle, polarity of the battery, distances etc.), came out to be very difficult. Oersted took, in his text, long pains to unequivocally describe even just one experimental setting and its results. The challenge became even more acute by the experimental finding that the needle was deflected in the opposite direction when the wire was below instead of above the needle. The fact that many emphasized that point so strongly and prominently is indicative for the serious challenge it raised to the concept of central force; a concept so prominent in all physics of the time.

With only little hindsight, the problem can be formulated as the challenge to correlate three directions: of electricity, of magnetism, and of motion. The first task was to define each of these directions for itself. Was the direction of electricity, e.g. to be taken as the direction of the wire or of something outside of it, of magnetism as the direction between the poles, of motion as the direction of rotation? Plus there were, as a second challenge, no geometric concepts available that would allow to express the correlation of three spatial directions in some generality. Hence the early electromagnetic experiments were usually described in terms of compass directions which made any generalization extremely difficult.

In my talk, I go through some attempts to cope with the problem. Oersted already took a significant step beyond tradition. He postulated an “electric conflict” that went on in circles outside the wire and perpendicular to it, or more precisely in spirals whose axis was the wire. To indicate the sense of rotation of those spirals, he adopted the botanic nomenclature of *sinistrorsum* and *dextrorsum* (left or right hand turn). Hence he was able to give a first idea of the geometrical constellation by imagining directions of electricity other than the wire and transferring spatial concepts from other fields.

The Paris mathematics professor Ampere started broad experimentation with the goal to establish a regularity for the deflection of the needle in full generality. Together with an instrumentmaker he devised new apparatus and framed new concepts that allowed him to comprise his results into a first law. His talk of electric current was explicitly introduced to denote directions (wire plus arrow), not physical processes. Starting from here, he introduced the concept of “left” and “right” of an electric current, and explicated it by extensive imagery: imagine a man, with the current running from head to feet, and his face turned towards the magnetic needle. In this constellation, the direction of his left and right arm would give the direction of “left and right” of the current, and the right direction was the one into which the north pole of the needle was regularly deflected. While Ampere made a personal sketch of the situation on paper, no such figure was given in his published papers. For the readers, the imagery was left to imagination.

After his discovery of an interaction of electric currents without magnets involved, Ampere abruptly switched his agenda and focussed on what he now called electrodynamics. Since mathematization was his central goal here, he essentially took recourse to established notions of central forces, but gave them a new twist since the centres of force – the current elements – had “direction arrows”, and

the force depended not only on the distance of the elements, but also on their relative constellation of directions. Hence again he had a geometrical problem, though a reduced one: the problem of relating two independent spatial directions (of two current elements). He treated the constellation in full generality by means of elementary geometrical tools. No special imagery was required here any longer, but rather analytic formulas, using trigonometric functions that in later versions would be reframed in directional derivations.

On the other side of the British channel, there was a different attitude visible. The culture of physical research was much less shaped by mathematics (in particular analytic methods) than in France. The brilliant chemist Davy developed, in order to study electromagnetism, special devices to investigate directions, such as a circular cardboard, with the wire running through its center and perpendicularly to it, and having iron needles distributed on it in circular arrangements. The device served to study the direction of magnetization induced by the electric current, and brought Oersted's idea of a circular geometry into material realization.

Davy's "chemical assistant" Michael Faraday, when asked to compile a historical sketch of electromagnetism, read everything he could get on the topic, redid all experiments, and tried to synthesize and generalize them as much as possible. His main point of attention was exactly the problem of direction, and he developed his own, mainly visual means to cope with it. For example, and for representing the relation of electricity and magnetic motion, he developed a glass bar, with two perpendicular directions marked on it in different planes that presented this relation constantly. When he subsequently started his own research, taking up a point that Ampère had left unfinished, viz. the analysis of asymmetric constellations of wire and magnetic needle, he developed new modes of presentation. His attempts of formulating a general account made extensive use of visual schemes that compressed many experimental findings in few figures, with perspectives often changing from top-down to side view, and attributing motion to sometimes to the needle, sometimes to the wire. As a result of this sort of reasoning with images, he found the most general account to be the assumption of a circular motion of the wire round a magnetic pole. The attempt to realize that assumption led him directly to the discovery of the first continuous electromagnetic rotation.

Ten years later, after his discovery of electromagnetic induction, and in attempting to formulate a law of how motion of a magnet led to induction, he was confronted even sharper with the insufficiency of existing accounts of direction. As a result of a long struggle to find a representation of the geometrical constellation that allowed to formulate a consistent law, and after repeated failures, he took a very unusual step: He tried to represent the direction of magnetism no longer by the axis of the magnet but by "magnetic curves", i.e. the pattern around magnets that had been well-known for ages but never given any physical meaning. The step was successful, in that he could not only formulate an induction law for magnets in motion, but also embrace all of his former induction experiments. For experiments with terrestrial magnetism, he only had to take the direction of the magnetic dip as the equivalent of magnetic curves. This success brought him even

to a point at which he was able to state in full generality the problem of direction and at the same time to present a solution. He sketched a tripod at right angles, and expressed a most general scheme of the relation of electricity, magnetism, and motion. “If electricity be determined in one line and motion in another, magnetism will be developed in the third; or if electricity be determined in one line and magnetism in another, motion will occur in the third. Or if magnetism be determined first then motion will produce electricity or electricity motion. Or if motion be the first point determined, magnetism will evolve electricity or electricity magnetism.” [Faraday’s Diary, entry of 26 March 1832, No. 403 [1] , p.425]. It is noteworthy that he had formulated here a solution of the problem of direction that had vexed electromagnetism since a decade. It is likewise important to note that this solution worked only when the direction of magnetism was taken to be the direction of magnetic curves – a highly unusual step that perhaps formed the reason why Faraday formulated that solution only in his private notes, but not in his published papers.

The short overview on the roles of visualizations in early electromagnetism shows that in all attempts of treating the problem of directions, visualizations and imagery played an essential role. It also shows that there was a large variation between different researchers, even when working on the same challenge and the same experiments. Visualization, moreover, was sometimes a temporary aid, just to be left behind as soon as possible. In other cases, it was essential to the process, to the degree that one should speak of visual reasoning, and became central feature of the result. The most striking case is Faraday and the long formation process of his most original concepts. One could go so far as to say that the origin of field theory was most closely connected to an unusually powerful and fruitful use of visual reasoning.

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### **What (if anything) Do Feynman Diagrams Represent?**

MICHAEL STÖLTZNER

Opinions about Feynman Diagrams have been controversial since they entered quantum theory in 1948. While Richard P. Feynman – at least initially – considered them as depicting actual or possible physical processes and as the core element of a particle-centered approach alternative to quantum field theory, Freeman Dyson integrated the diagrams into this theoretical framework and understood them as the pictorial expression of individual terms in a perturbative expansion of a scattering process. This plurality of interpretations has persisted until today. It reaches from theoretical physics textbooks that warn against treating Feynman diagrams as anything but a bookkeeping tool to experimental physicists’

pronouncements to have measured perturbative corrections up to a certain order in atomic energy levels or the depiction of a certain type of particle interaction studied in a contemporary collider experiment, such as LHC's announcement of the discovery of a Higgs boson in July 2012.

My paper investigates whether the recent philosophical debates about models in the sciences can be employed to develop a suitable concept of what Feynman diagrams represent. The primary motivation to do so is the surprising resilience of Feynman diagrams within a research field in which what Peter Galison [4] has called the image tradition has increasingly become a mere mode of presenting results, while the validation of experimental results comes from a complex logical and statistical analysis that compares events identified on the basis of a multi-step trigger menu against the background of already known physics. Even though scattering events are thus recreated from the data of many data points within the detector, the presentations of ATLAS and CMS heavily use Feynman diagrams to represent physical processes, the individual channels from which a Higgs boson is created and along which it decays almost instantaneously.

In my presentation I first discuss David Kaiser's [5] analysis of the early history of Feynman diagrams that initially came in diverse forms that were often connected to the experiment a theorist was associated with. Rather than representing physical processes, the diagrams functioned as a paper tool that continued earlier visual representations, among them Maxwell's lines of force and bubble chamber photographs. It is essentially these traditions that keep together, on Kaiser's account, the initially rather diverse family of diagrams. While this might be a good explanation of their use in the 1950s and 1960s, it appears to me that today the image tradition has largely lost its validating role. Even though dealing with bubble chamber data effectively required sophisticated statistical methods and a detailed knowledge of ionization processes, current analyses of detector background involve many more steps of modeling. (See [3] for a history of these strategies.) These developments, it seems to me, also led to a more abstract role for Feynman diagrams.

Wüthrich [8] sheds closer light on the historical development. Feynman was initially motivated by the model of a quivering electron that was then taken as a physical interpretation of the Dirac equation, but he later understood that the local Hamiltonian picture was physically meaningless and that in the local region one might insert alternative processes according to what became the Feynman rules. Wüthrich sees these rules not primarily in a pictorial tradition but also in the tradition of the tables of possible processes and term schemata that were used in atomic and, from the 1930s onward, in nuclear physics. Both traditions, to Wüthrich's mind, are not inconsistent. "Feynman showed that only electrons behaving in a way that is allowed by the Dirac equation could be represented. This makes his representation a model, or model system, of the Dirac equation, in the sense, albeit roughly, of present-day philosophy of science (Cartwright, Giere)." ([8], p. 178) Wüthrich even argues that Feynman diagrams may well provide an instance where "problems are not solved in the usual sense of the word but are

rather made to disappear by using a symbol system that appropriately represents an adequate model.” ([8], p. 189) Wüthrich’s introduction of the notion of model is possible not least because the representative commitments characteristic of the contemporary understanding of models are less stringent than ontological commitments to real or virtual particles occurring in Feynman diagrams.

In the ‘Models as Mediators’ [6] approach, which I find most instructive for present debates, models are autonomous because they function – and often are construed – partially independent of any high-level theory and thus develop representative features in their own right without referring to distinct entities. What is required though is that they act as measurement devices in the sense that the models’ adequacy can be tested. In [7], I have used this approach for an analysis of the variegated model landscape of elementary particle physics that includes the standard model – now often considered a successful theory – and the models going beyond it, many of whom are formulated and analyzed by means of Feynman diagrams. Wüthrich’s second remark seems to indicate an even further departure from ontological commitments in the traditional sense because it effectively takes Feynman diagrams as a model that is no longer an idealization of, or approximation to, any real referent. This diagnosis, which Wüthrich does not spell out in detail, in my view resembles the use of minimal models in quantum field theory that Bob Batterman has analyzed at several examples. Such minimal models – e.g. integral models in quantum statistical mechanics – are explanatory not because they share some common features with a target system, but “because of a story about why a class of systems will all display the same large-scale behavior because the details that distinguish them are irrelevant.” ([1], p. 349) They proceed by first “showing that various factors are irrelevant. The remaining features will then be the relevant factors.” ([1], 363) It is true, Batterman’s minimal models derive their role from specific, well understood mathematical properties which are unavailable in large parts of elementary particle physics – the only exception being the renormalization group.

But I still think that this mathematical and abstract aspect of Feynman diagrams also allows us to better understand them as a model. For the goal should not be to employ a complex notion of model in a universal manner. After all, the whole model debate of the past 20 years was a rejection of an exclusively logic-oriented approach. What is rather required is – as Moritz Epple has put it in his keynote address – to employ them as a precisely specified actors category. This makes models a more flexible philosophical category, such as explanation. My thesis is, along these lines, that the issue of representation cannot be addressed in a universal fashion that covers all aspects of the physicists’ employment of Feynman diagrams. There is a difference between (i) a single Feynman diagram, (ii) the grouping of a certain type of diagrams that corresponds to certain types of physical processes that physicists set out to measure as effects, (iii) the expansion up to a certain order that physicists treat as loop corrections, and (iv) the whole series which – mathematical problems aside – is considered as a faithful expression of the physical scattering process as a whole. Case (iv) represents of course an

unreachable ideal that would be rendered meaningful only after a proof of the convergence of the perturbation series arising from the Feynman rules, which is not in sight not even by availing oneself of the distinction between convergent series and approximations that has been emphasized by Michael Berry, in the physical side, and Batterman, as regards its philosophical consequences. Case (i) basically amounts to a diagrammatic representation of a mathematical object, a term in the perturbation series. It cannot be isomorphic to any real physical process precisely because there will be higher order processes that are described by the Feynman rules.

But a single diagram may well stand for a certain group of processes in the sense of (ii). The diagram then corresponds to a sub-series of the whole perturbation series, the depicted process being the leading order sub-process. Such a classification can be motivated by a mathematical or a physical property that determines the diagram's representative features. Such cases are not uncommon in the history of mathematical physics and they may be seen as a mathematical analogue to the term schemata that Wüthrich has considered as among the motivations for Feynman diagrams. Take, for instance, the Lissajous figures that represent terms in the expansion of a superposition of sinus waves or the orthogonal polynomials solving the Schrödinger equation. But also experimental physicists are using various sub-series – typically up to a certain order – as denoting effects and corrections. Take for instance the table in ([2], p. 113) that lists, among others, one-loop vacuum polarization, recoil corrections, proton size, two-loop corrections.

Not all infinite series can be rearranged in an interesting way; take sine or cosine. But all series expansions can be stopped at a certain order in the sense of case (iii). In this case, it is a specific property of several Feynman diagrams, loop order, that qualifies them for consideration by physicists. The Feynman diagram here rather functions as a calculational tool determined by the calculational power in evaluating them and the experimental precision that can be reached, rather than exhibiting representative features. But it is still a measuring device in the sense that it provides the partially autonomous point of contact between theory and data along the lines of the 'Models as Mediator' approach.

Let me conclude that such a plurality in a model is not necessarily a problem if there is a coherent tradition of its use as an actors category that includes awareness of the complex representational features. While in the early days (cf. [5]) there were quite a few mishaps and *bona fide* disagreements, the practice of Feynman diagrams has now been well established, including the identification of their mathematical shortcomings. But they are not a mere paper tool, but also a model that partakes in the physical and mathematical realm, not only as the symbolical representation of a model system, such as the quivering electron, but also by continuing several pictorial traditions on mathematical physics. Admitting the plurality in the representative features of Feynman diagrams, if understood as a model, does not prevent us from using the notion of model as a basis for a unified approach to Feynman diagrams.

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## Henri Poincaré and his "model" of hyperbolic geometry

PHILIPPE NABONNAND

The aim of the talk is to trace how and when Henri Poincaré used non-Euclidean geometries (NEG) in his mathematical and philosophical works, with a particular attention to the genesis and the description of his "model". We begin by a short presentation of the context of NEG in France around the 1870-80s. Then we expound from several sources the introduction and use of NEG in Poincaré's work about Fuchsian functions and we stress on the analogy between elliptic functions and fuchsian functions.

The context of non-Euclidean geometry in France around the years 1870–80s At the end of 1869, Jules Carton sent to the Academy of Sciences of Paris a "proof" of the postulate of parallels. One of the leaders of the Academy, the well-known mathematician Joseph Bertrand, approved this proof. This announcement and this almost official approval provoked a series of proposals for proof of the postulatum but also many criticisms, first, expressed in the privacy of correspondence, but then quickly in newspapers such as the review published by the Abbot Moigno, *Cosmos*.

Others, like Jules Hoüel and Gaston Darboux, saw it as an opportunity to popularize and deepen the debate. Jules Hoüel was the translator in French of the major texts of non-Euclidean geometries. Hoüel fought for the acceptance and the recognition of NEG in a context of discussions about the provability of the axiom of parallels, the consistency of NEG and the status of the axioms of geometry. His point of view was moderately empiricist.

Since 1875, there had been a reception of NEG and a debate in the field of Philosophy via the *Revue philosophique de la France et de l'Étranger*. Founded by the psychologist Théodule Ribot, the *Revue* gave special attention to contemporary debates on philosophy of science, with a focus on NEG and the status of axioms of geometry. Many actors contributed to this debate, mathematicians, engineers,



psychologists-physiologists. In this context, the *Revue* stressed the importance of German theories in experimental psychology, especially about "spatial sense".

The three "Suppléments" In 1880, Henri Poincaré took part in a competition announced by the French Academy in 1878. The subject was "To perfect in any material respect the linear differential equations theory with a single independent variable". First he submitted a memoir to which he later added three "suppléments"<sup>1</sup>.

In the first one, Poincaré studied the behavior of the quotient  $z = \frac{f(x)}{g(x)}$  of two independent solutions of a linear differential equation of order 2 and asked the question to know when  $x$  is a meromorphic function of  $z$ . In this intention, he described a subgroup of transformations of  $PGL(2, \mathbb{R})$  and an associated tessellation (paving) of the unity disk. Poincaré stressed the link of these geometrical considerations with the hyperbolic Geometry. For this goal, he identified the group of transformations he studied and the group of the "pseudogeometry" of Lobatchewski. In fact, he will made a very moderate use of the "convenient language" but at the end of the first supplement he introduced a seminal remark which he would thereafter consider as the core of the use of NEG in the theory of Fuchsian functions, the analogy between elliptic functions and Fuchsian functions.

In the *Report on his own works* [[5], he explains the crucial nature of the use of NEG in the theory of Fuchsian functions as resulting from the analogy elliptic functions/Fuchsian functions. The analogy breaks down as follows:

Euclidean geometry	Discrete subgroups of orthogonal group	Lattices	Elliptic functions
Non-Euclidean geometry	Discrete subgroups of $PSL(2, \mathbb{R})$	Hyperbolic pavings	Fuchsian functions

*Abstract:*

- (1) Few drawings considered in the context of geometry of the unity disk.
- (2) Identification of the groups of transformation = identification of the geometries.
- (3) The identification of geometries provides a convenient language.
- (4) The thema of the analogy elliptic functions/Fuchsian functions.

In the second supplement, Poincaré gave a definition of the elements of pseudo geometric plane in terms of classical geometry of disc unity. He described also the group of pseudo geometric movements in terms of homographies which set the fundamental circle.

The first half of 1881 In a talk about "applications of NEG to theory of quadratic forms" [2], Poincaré uses, despite the title, the same exposition mode as in the "suppléments". He first studies the linear transformations (with integral coefficients) which preserve a ternary quadratic form (with integral coefficients).

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<sup>1</sup>These three "suppléments" were discovered in the Archives of Academy of Sciences of Paris by J. J. Gray in 1995 and edited, with an introduction, in 1997 by J. J. Gray and S. Walter [1].

Following Hermite and Selling, he is led to investigate the geometry of tessellations of unity disc. After a classical description of the geometry of the group of substitutions that exchange regions of the tessellation, he finds it convenient to use the vocabulary of the pseudo geometry.

*Abstract:*

- (1) Identification of geometries as identification of elements.
- (2) The identification of geometries provides a convenient language.

Poincaré published eight notes about Fuchsian functions during the first half of the year; only three of them mention NEG. This raises a question: are NEG really important for Poincaré's theory of Fuchsian functions? Poincaré's answer is ambivalent. He emphasizes that NEG are very important for the discovery process but he doesn't really use NEG in his papers [3].

In a note about Kleinian groups (published July 11th, 1881) [4], Poincaré copes with the question of finding discrete subgroups of  $PSL(2, \mathbb{C})$ . Of course, finding Kleinian groups is a more general problem than finding Fuchsian groups, which are discrete subgroups of  $PSL(2, \mathbb{R})$ . Once again, Poincaré explains how NEG are important in the discovery process without translating it explicitly in the exposition of theory. In this paper, he gives a description of hyperbolic geometry on a half-space (3-dimensional hyperbolic geometry). In his paper on Fuchsian groups in *Acta mathematica*, Poincaré evokes NEG in the same terms.

*Abstract:*

- (1) A claim that NEG was important for the discovery of Fuchsian and Kleinian groups.
- (2) A new identification of elements.
- (3) No real use of NEG

**Conclusion** In a paper entitled 'Les géométries non euclidiennes' [6], Poincaré claims that his dictionary is a proof of the non-contradiction of hyperbolic geometry. In this context, we can say that the half plane of Poincaré is a model (in the logical sense<sup>2</sup>) but we have to notice that the translation of axioms of NEG is not explicite (perhaps, included in the claim concerning all the theorems).

In any case, Poincaré made a very moderate use or no-use of the "convenient language" in mathematical papers. In particular, there is no drawing when dealing with NEG. Nevertheless, referring to the analogy between elliptic functions and Fuchsian functions, he claimed that hyperbolic geometry played a crucial role in the process of discovery.

Following the differentiation between structural analogy (correspondence between relations) and functional analogy (correspondence between elements which have analogous properties), we can notice that the functional part of the correspondence in Poincaré's dictionary of Poincaré is explicit and that the functional part is implicit (excepted when Poincaré refers to isomorphism between groups); nevertheless, Poincaré's conclusions (correspondence between theorems) are true if the analogy is structural.

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<sup>2</sup>If a deductive system has a model, the system is semantically consistent.

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### Mathematical Milky Way Models from Kelvin and Kapteyn to Poincaré, Jeans and Einstein

SCOTT A. WALTER

Following William Thomson's calculation in 1901 of the Milky Way radius [9] and J. C. Kapteyn's announcement [5] at the Congress of Science and Arts during the World's Fair in Saint Louis of his discovery of two star-streams (1904), Henri Poincaré realized the interest of kinetic gas theory for modeling astronomical and cosmological phenomena. Soon others followed, including A. S. Eddington and Karl Schwarzschild, who proposed dualist and unitary models, respectively, of the observed stellar velocities. Eddington [1] affirmed Kapteyn's two-stream hypothesis on the basis of his analysis of the Groombridge stars, and claimed the streams were characterized by Maxwellian distributions with different constants. Shortly thereafter, Schwarzschild [8], on the basis of a different dataset, affirmed that there were not two star-streams but rather an ellipsoidal velocity distribution. The two models were judged at first to represent the data equally well, and further efforts were called for to determine which was best.

What Eddington and Schwarzschild provided in 1906–1907 were mathematical representations of empirical data. Neither Eddington nor Schwarzschild took up Poincaré's suggestion that the Milky Way was undergoing a rotation [6], at least not explicitly. Poincaré developed this bold conjecture in his Sorbonne lectures of 1910–1911 [7], the publication of which constituted the first theoretical treatise on cosmology. Notably, in his treatise Poincaré derived the virial for the case of a gaseous mass with Newtonian attraction, and took up the mixing problem. Like Poincaré, James Jeans challenged belief in the stationary state of the universe, based on his calculation of the angle of deflection of colliding stars [4]. A "stargas" (Sterngas) model of globular nebulae was investigated by Einstein in 1921 using Poincaré's virial, presumably as a way to fix the value of the cosmological constant he had introduced in 1917 to the field equations of general relativity [2], and to obtain thereby an estimate of the size of the universe [3].

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### Mach’s Principle and Relativistic Cosmology, 1917–1924

DAVID E. ROWE

This period marks the beginning of relativistic cosmology, which has normally been discussed in terms of two competing models: the “cylinder universe” of Einstein and the matter-free world of de Sitter. The term cosmological model only became common, however, after around 1933 when it was used in a well-known review paper written by H. P. Robertson. Einstein and de Sitter were concerned with finding static solutions to the field equations with “cosmological constant.” In Einstein’s case his universe aimed to implement what he called “Mach’s Principle,” a notion de Sitter rejected as pure speculation. The latter’s matter-free universe flew in the face of Einstein’s claim that the matter-field alone induced inertia, sparking a famous debate. Hermann Weyl and Felix Klein soon entered into this controversy, though in quite different ways. The period ends with Weyl’s amusing dialogue, published in *Die Naturwissenschaften* in 1924, in which the debate is re-enacted as a theological discussion over the dogma of Mach’s Principle as a condition for membership in the “church of relativity.”

Einstein and de Sitter had already discussed the implications of general relativity for cosmology in 1916 when Einstein visited with him in Leyden. Einstein’s first attempt to introduce a relativistic cosmology was based on a flat global space-time in which he let the gravitational potential become infinite at spatial infinity. Arguing against this, de Sitter noted that this assumption could not be made independent of the choice of coordinates, a point Einstein conceded in early 1917. It was then that he unveiled his famous “cylinder universe”, a space-time geometry

admitting a foliation with space-like 3-spheres of constant radius. Immediately thereafter de Sitter showed that Einstein's new cosmological equations admit a vacuum solution, which he also treated as a static universe. Einstein was not amused, and the following year he went on a counter-attack, claiming that the de Sitter solution contained singularities at the boundary of the coordinate system, which he interpreted as an indication that it was not a matter-free solution after all. Einstein was sure that matter lay hidden just over the horizon where the metric degenerated. Even after he admitted (in private) to Felix Klein that de Sitter's space-time was free of singularities, he completely ignored it as a viable cosmology throughout this period.

During these years, Einstein was a steadfast defender of Mach's ideas regarding the relativity of inertia. Mach had famously advanced the notion that the inertial properties of matter were due to some kind of interaction with distant masses; this served as part of his critique of Newton's notions of absolute space and time, two of the cornerstones of Newtonian mechanics. Already in 1912 Einstein took up this Machian program in the context of a field-theoretic approach that coupled gravity and inertia. Indeed, this Ansatz precedes his early work on general relativity, which only began in 1913 when he began collaborating with Marcel Grossmann. By 1918 Einstein gave a new formulation of what he called *Mach's Principle*, according to which the gravitational field (as expressed by the metric tensor  $g_{\mu\nu}$ ) must be solely determined by the energy-matter field (given by the tensor  $T_{\mu\nu}$ ).

Historical and mathematical details connected with the Einstein-de Sitter debates can be found in the references cited below. In the talk that followed mine, Erhard Scholz took up Hermann Weyl's part in this complex story, showing how Weyl shifted his support from Einstein to de Sitter during this period. Weyl's initial enthusiasm for Einstein's theory had abated somewhat by 1924, reflecting his more sober attitude regarding bold attempts to capture all of physics through a beautifully constructed field theory. The fact that none of the actors from this time invoked the now common notion of cosmological models also suggests the persistence of a still lasting belief in something akin to a Leibnizian pre-established harmony between mathematical theories and physical reality.

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### Weyl on cosmology: (re-)presenting the “world in the large”

ERHARD SCHOLZ

H. Weyl got involved in the ongoing debate between Einstein and de Sitter cosmology in 1918 during his work for *Raum, Zeit, Materie* (RZM). His developing thoughts on the subject are well documented by the changes of the chapter “The world in the large” in the successive editions, most essentially the 4th ed. 1921 (translated into English by H. Brose) and the 5th ed. 1923. All in all he stated his views on cosmology in the time period between 1918 and (roughly) 1934. He started off as a staunch defender of Einstein’s view on cosmology, in particular the *Mach principle* as Einstein understood it at the time (see D. Rowe’s talk, this conference), shifted toward a more serious acceptance of de Sitter’s framework in 1922 (between 4th and 5th ed. of RZM), and developed some ideas of his own, preparing the later “cosmological principle” (specification of a timelike pencil of geodesics as crucial element of cosmological models) and own investigations on cosmological redshift. In a side-remark he questioned the fundamental nature of the underlying “kinematical” explanation of the latter.

With regard to the central topics of this conference, it ought to be mentioned that Weyl was one of the first authors (perhaps the first one?) to explicitly use the terminology of “model” in the context of non-Euclidean geometry (“Kleinsches Modell” or “Euklidisches Modell” in his report on NEG in chap. II of RZM). Moreover he took up the language of physicists when talking about the “Bohrsche Atommodell”. In cosmology, however, he consistently chose the much more realistic language of “world” or “universe” (even when discussing the alternative between Einstein’s and de Sitter’s view).

Although Weyl was not convinced by Einstein’s boundary value argument (“in the infinity”) for his “cylinder world”,<sup>1</sup> he joined the more general program of the latter to “derive” the metric of a (at the time of “the”) cosmological solution from the mass distribution in the universe. Weyl studied generalized “cylinder

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<sup>1</sup>Weyl hinted at the non-elliptic nature of the Einstein equation specific solutions of which have to be determined by initial values on a spatial hypersurface (later: Cauchy problem), not by boundary values.

solutions”, i.e. spherically symmetric solutions with varying mass density, in particular those with a mass band close to the “infinite horizon”, as his contributions to Einstein’s strong Mach program of the time (RZM, 1st ed.). Weyl started to discuss De Sitter’s “world” (model) in the 4th ed. of RZM, with an input from Klein’s global characterization of the de Sitter manifold as the unit sphere in 5-dimensional Minkowski space. There a “wedge shaped” submanifold could be identified with both, a generalized “cylinder solution” with vanishing mass density, and with the original coordinate description of de Sitter’s “world” (showing their mathematical equivalence). In this geometric approach it was easy to show that the orthogonal timelike trajectories to spacelike hypersurface were *not geodesical*. This observation was already made by de Sitter; he interpreted it as a (tentative) explanation for the observations indicating a systematic bias of the spectral shifts of *nebulae* (Lundmark, Slipher, later: cosmological redshift).

Weyl (partially) *visualized* this constellation in RZM, 5th ed., by a 2-dimensional graphics of the “wedg shaped” segment of the de Sitter manifold, indicating the (projected) spacelike foliation by radial family of straight line segments (a later widely used picture). His own view was that one ought to consider a pencil of timelike geodesics forming a common “Wirkungsgebiet” (an asymptotically past causally connected region) which had an easy geometrical description by cutting the planes through a generating line of the asymptotic conic to the de Sitter manifold (in Minkowski 5-space). Also this *Wirkungsgebiet* could be visualized by a plane graphic which indicated the geodesic timelike flow by lines on the projected hyperboloid. Weyl favoured a representation of “the world” by combining this hypothesis (later “Weyl’s hypothesis”) with the static spacelike foliation of de Sitter’s wedge (our terminology: *Weyl’s model*).

This timelike flow produced a systematic shift of spectral lines to the red for observers on the static (wedge-like) foliation. Weyl indicated how to calculate the arising cosmological redshift systematically. He derived a formula and evaluated Lundmark’s data of 1914. This allowed a numerical fit of the only free parameter of the model, the curvature “radius”  $a \approx 10^9 ly$  of the de Sitter hyperboloid. A linearization of Weyl’s redshift formula leads to  $z \approx a^{-1} \cdot d$  with the distance  $d$  of the source (moving on the timelike pencil of geodesics) and the observer, measured in the static space of the observer (at the time of observation). A comparison with Hubble’s value for the coefficient of his *law*,  $z = H \cdot d$ , shows that Weyl’s  $a^{-1}$  lies ca. 60% above Hubble’s value of 1928 (both one order of magnitude “too high”, due to a systematic error in the distance determination of nebulae).

The reliance on the de Sitter hyperboloid was no longer compatible with Einstein’s strong Mach program. Weyl presented his farewell to the Mach principle in a beautiful article *Massenträgheit und Kosmos* addressed to a wider audience [6]. In 1930 he took up the discussion of cosmological redshift connecting his view with the then rising models of seemingly expanding spacelike folia, in particular the Lemaitre-Robertson “coordinates” of flat, exponentially warped spacelike sections derivable from the de Sitter manifold [7]. Weyl agreed that for a while one ought to pursue this “kinematical” hypothesis for cosmological redshift, although

he agreed with F. Zwicky that, in due time, one might better look for a “more physical”, i.e. field theoretical, explanation.

The shift from representing “the world” by simple cosmological solutions of the Einstein equation to discussing “models” happened around that time [3]. Weyl did not participate in this new phase of cosmological modelling, with the exception of some minor comments in the English edition of *Philosophy of Mathematics and Natural Science* (1949).

Aside from Weyl’s original papers, in particular [4, 5, 6, 7], the talk relies on the splendid work of H. Goenner [2] and BergiaMazzoni [1].

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### Examples of recent use of 19th century geometric models

IRENE POLO-BLANCO

In this talk we discuss some of the geometric models present at the University of Groningen (The Netherlands). These models are often made of plaster, string or cardboard and represent certain geometric objects (such as curves, surfaces or polyhedra). We discuss the models from both a historical and a mathematical perspective as it was reported in the speaker’s thesis [6]. We also show some examples of how the models have been used recently.

The building and use of mathematical models and dynamical instruments for higher education received a new impulse in the nineteenth century in Europe [3]. We find an example of this at the polytechnic schools in Germany during the second half of the nineteenth century where collections of mathematical models were constructed ([1] and [2]). Ludwig Brill, brother of Alexander von Brill, began to reproduce and sell copies of some mathematical models and founded a firm in 1880 for the production of models. This firm was taken over in 1899 by Martin



Schilling who renamed it. Schilling's 1911 catalogue [11] describes forty series consisting of almost four hundred models and devices and contains the name of the models, a short mathematical explanation and, in some cases, a drawing of the model (see Figure 1). There are also mathematical texts explaining each of the series of models ([12]).

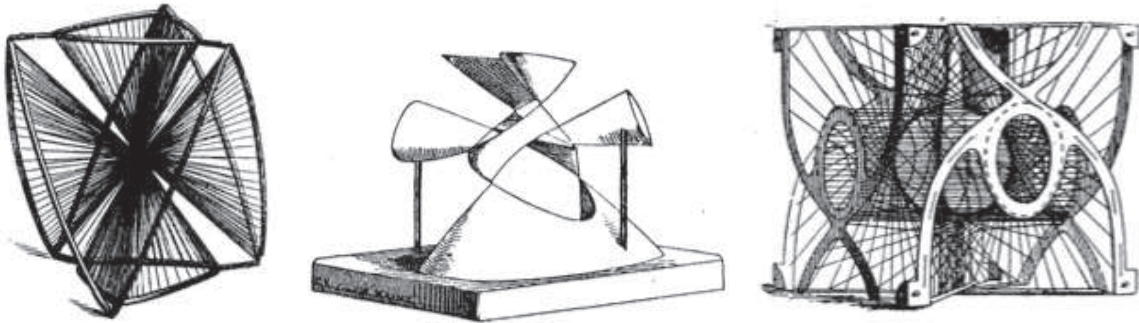


FIGURE 1. Drawings of models in Schilling's catalogue in [11]

We discuss the role of Klein in the construction and popularization of the models and provide examples of some of the series in Schilling's catalogue. We emphasize how the study of these models can help retrieve classifications of curves and surfaces. In order to show this, we present several examples. We discuss the plaster models from series XVII and the string models from series XXV, both displaying a classification of plane curves of degree 3 by Möbius (see [4]). We also present the classification by Rohn of ruled surfaces of degree four ([10]), and show string models of some of the cases in his classification (see Figure 2). Rohn's work has been studied in detailed and compared with the classifications provided by Cayley and Cremona in [9].

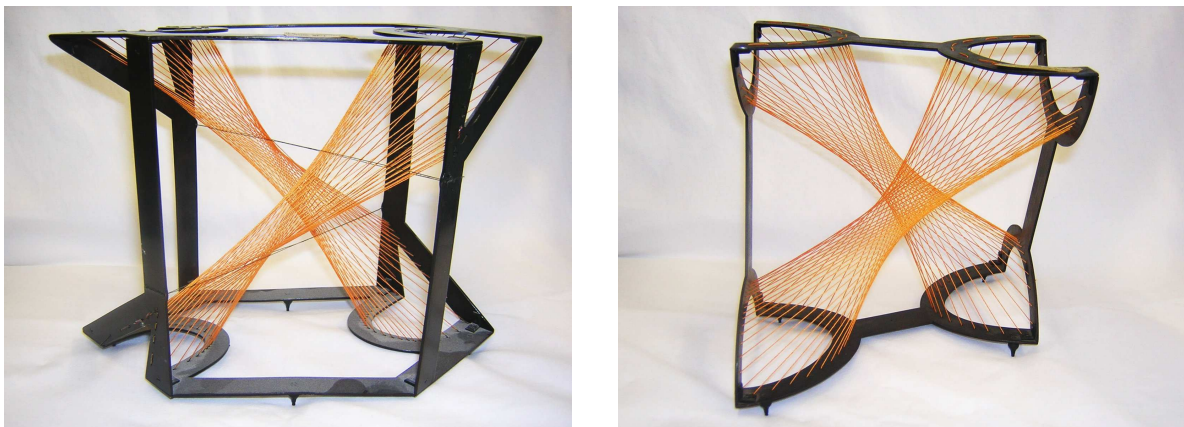


FIGURE 2. Models of ruled surfaces of degree four with two real lines (left) and with two complex conjugate lines (right)

We find other examples of recent use of the models as a resource for mathematical research that have lead, for example, to master thesis (see e.g. [5]). Classical

models of surfaces have also served as inspiration for artists. As an example of this, the work of the Spanish sculptor Cayetano Ramírez concerning the construction of models of cubic surfaces is presented (see [obratano.com](http://obratano.com)).

We end the talk by discussing some cardboard models of polyhedra made by the amateur mathematician Alicia Boole Stott present at the University of Groningen. After briefly discussing Boole-Stott's life, her drawings and models are displayed explaining how they relate to the three-dimensional sections of the four-dimensional polytopes (see [8] and [9]). This is another example of how models can be used as a tool for visualization, in this case, to help visualize four-dimensional objects.

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### Beltrami's model between mathematical proof and actual representation

ROSSANA TAZZIOLI

Beltrami elaborated his actual representation of the hyperbolic plane in a mathematical context far from nineteenth century logic and its language. Beltrami's aim was indeed to build a surface (the pseudosphere) embedded in a Euclidean space where geometric theorems and results of the Bolyai-Lobachevski plane can be easily interpreted. In the talk, we focused on Beltrami's paper *Saggio di interpretazione della geometria non-euclidea* [1] published in 1868, and showed how Beltrami deduced his representation. As he wrote in the introduction of his paper:

We tried to explain the results to which this theory leads; and, by a procedure which follows the good scientific traditions, we tried to give it a real basis [*substrato reale*].

However, Beltrami's interpretation of the Bolyai Lobachevskij plane on the pseudosphere is valid only locally, as Hilbert proved in 1901 [8]. In 1869 Beltrami built a paper-model based on the results deduced in his *Saggio*, where one can actually draw figures and so interpreted results of the hyperbolic plane geometry. On March 13th, 1869 he communicated this idea to his French colleague Jules Hoüel<sup>1</sup>:

I had, in this period, a strange idea I communicate to you [...] I wanted to try to actually build the pseudosphere on which one realizes the theorems of non-Euclidean geometry. [...] And since this surface is the same [it has constant curvature], the pieces of paper are exactly equals one to each other: their assembly should then reproduce approximately the surface.

Then Beltrami explicitly explained his construction, and was proud to notice that figures drawn on his model led to mathematical relations in accordance to the well known formulae of hyperbolic geometry. On April 22th, 1869 he wrote to Hoüel:

I have also drawn on my surface two geodesics, parallel to the same geodesic and passing at the same point: the angle between them is about 100 degrees and it has with the distance of their intersecting point the relation established by Lobatcheffsky.

Furthermore, the material construction of his model induced him to guess some theorems of elementary geometry. For instance, in a letter to Hoüel (dated 13 March 1869) he wrote:

You speak of empirical propositions that can be found by this means [the model], and you are perfectly right, in fact there are surfaces of which we do not know their general equations. Here you are a proposition I have began to realized: A pseudosphere can always be folded such that any of its geodesic lines becomes a straight line. I give this to you only as an approximative result that is produced when, holding firm with both hands two points of the flexible surface, it is stretched as far as possible without tearing it. This result was even more striking to me because I supposed the opposed...

Beltrami's intuition concerns what in 1901 Hadamard [5] denotes a (*totally*) *geodesic surface* defined as follows: if any segment of geodesic joining two points belongs to the surface, then all the geodesic belongs to the surface. Surfaces with constant curvature are geodesic surfaces. Beltrami remarked that a segment of a geodesic can be transformed in a straight segment by stretching his model without tearing it. Since the pseudosphere (actually represented by his model) is a constant curvature surface, that means that the whole geodesic can be transformed in a straight line.

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<sup>1</sup>The letters by Beltrami to Hoüel are published in [2].

Beltrami sent his model – nowadays kept at the Department of Mathematics, University of Pavia – to his friend Luigi Cremona with a letter (dated 25 April 1869) containing some explanations about it:

I give you some instructions about what you should do when you receive this box, since it contains something special in shape and nature. You should pay attention when you break the external paper [...] The surface is folded as a surface of revolution and its aspect is the following [...] The surface of revolution [...] is that to which equation (14) of my Saggio di interpretazione [the line element of the pseudosphere] refers to [...]<sup>2</sup>.

Beltrami's actual model was also useful in order to convince mathematicians who were opposed to non Euclidean geometry – such as Placido Tardy, Domenico Chelini, and Giusto Bellavitis – that the new geometry is correct and intellegible. For instance, Beltrami wrote to Chelini on August 7th, 1868:

I just sent to Battaglini a paper aiming at reconciling non-Euclidean geometry with classical geometry, at least in certain limits. I hope you are persuaded that – if we give the interpretation I explained – all theorems of this geometry [non-Euclidean geometry] are evident and belong to the geometrical concepts we all admit. On the contrary, if we consider the same theorems *à la lettre* they become unintellegible.<sup>3</sup>

Therefore Beltrami used his model not only as a research tool, but also as a propaganda, since visualizing hyperbolic plane geometry was a compelling argument for the spread of non-Euclidean geometry.

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<sup>2</sup>This letter is published in [3]. Moreover, this paper explains the construction of the pseudosphere by folding Beltrami's model with mathematical details

<sup>3</sup>The letters by Beltrami to Chelini are published in [4]

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## Plateau and surfaces

JEREMY GRAY

This talk looked at the mathematical and experimental study of capillarity and related topics from Laplace to Plateau, and at the independent advances in the theory of minimal surfaces with prescribed boundaries made independently by Riemann, Weierstrass, and Schwarz in the 1860s.

This work is joint work with Mario Micalef (University of Warwick) that we hope to publish shortly, and where full details will be given.

Laplace in 1806 (see several papers in (Laplace 1912)) made a theoretical study of capillarity, and concluded that the shape of a liquid surface in a narrow tube was determined by this equation

$$(1) \quad (1 + q^2)r - 2pqs + (1 + p^2)t + \frac{1}{a}(1 + p^2 + q^2)^{3/2} = 0,$$

where

$$\frac{1}{a} = \frac{1}{R} + \frac{1}{R'},$$

where

$$p = z_x, \quad q = z_y, \quad r = z_{xx}, \quad s = z_{xy}, \quad t = z_{yy},$$

$R$  and  $R'$  are the extreme radii of curvature at the point in question, and  $(1 + p^2 + q^2)^{1/2}$  is the element of area of a surface given in the form  $z = z(x, y)$ .

In particular, the first variation of area is zero if this expression is zero, in which case  $R = -R'$  – so the surface has zero mean curvature – and

$$(1 + q^2)r - 2pqs + (1 + p^2)t = 0. \quad (MSE)$$

This was already known as the equation for a minimal surface – it follows immediately from Lagrange's (1761) – but very few solutions to it were known. In particular, and contrary to what is often stated, the helicoid and the catenoid were first discovered by Meusnier in his (1785).

Delaunay (1841) investigated equation (1), which expresses the problem of finding the greatest volume enclosed by a surface of given area. He was able to solve it when the surface is a surface of revolution – the surfaces he found are these days called Delaunay surfaces. They are, as he explained, obtained by rotating curves called roulettes, which are defined as follows: when a conic section rolls along a straight line; the locus of a focus is a roulette.

Each type of roulette is then rotated around its axis to generate a surface of revolution: The ellipse generates the unduloid, the parabola the catenoid, and the hyperbola the nodoid.

The names for the surfaces generated by the roulettes are due to Joseph Plateau (1801–1889). He was a distinguished Belgian scientist, and almost completely blind in the 1860s when he embarked on a series of experiments on soap films

spanning various wire shapes (called contours or boundaries). His many papers, later published as a two-volume book (1874) describe in careful detail a number of experiments on soap films and on liquid films floating in a different liquid of equal density, often constrained by various boundaries.

His procedure was lead with the theory, typically an informal, but mathematically aware study of mean curvatures. He compared the best mathematical theories with his observational findings, and then proceeds to novel observations by, for example, varying the initial state. Very often his interest was in the stability of the shapes that he found.

As part of his findings he also provided simple pictures to illustrate the unduloid and the catenoid. It is well-known today that two equal circles arranged in parallel planes so that the line joining their centres is perpendicular to the planes can bound a soap film in the shape of a catenoid when the circles are close together, but only two separate discs when the circles go beyond a certain distance apart.

But, as Plateau discussed, there is also an unstable catenoid that spans the circles. It lies inside the first and, as Plateau showed, when the rings move further apart, the catenoids move together until they coincide when the distance between the rings becomes a little more than  $2/3$  times their diameter. Beyond that, there is no catenoid that spans the rings and the catenoidal soap film collapses into two flat discs spanning the circles.

Stability of an extremal surface is controlled by the second variation of its area, and so today is an aspect of the calculus of variations (of course, in the least area problem there cannot be a maximum, so only the minimum can arise). However, in the 1860s this branch of mathematics was poorly understood. Even the implications of the vanishing of the first variation of area were poorly understood for a time.

However, it seems clear that the mathematical breakthrough in the study of minimal surfaces was made independently, and came from signal advances in the theory of complex analytic functions made by Weierstrass and Riemann. The key insight each came to was that minimal surfaces are provided by conformal harmonic maps, and conformal harmonic maps are intimately linked to complex analytic functions.

On Weierstrass's side, this culminated in the The Weierstrass–Enneper equations, see (Weierstrass 1866) . These give a representation of the  $x, y, z$  coordinates of a surface of zero mean curvature as the real parts of three complex functions (in terms of two complex functions  $G$  and  $H$  satisfying modest constraints here omitted):

$$(2) \quad x = x_0 + \Re \int_{u_0}^u (G(u)^2 - H(u)^2) du ,$$

$$(3) \quad y = y_0 + \Re \int_{u_0}^u i (G(u)^2 + H(u)^2) du ,$$

$$(4) \quad z = z_0 + \Re \int_{u_0}^u 2 (G(u)H(u)) du .$$

In the late 1860s, Weierstrass and still more Schwarz made extensive investigations into how a surface of zero mean curvature can be found with a prescribed boundary (they are summarised in Weierstrass *Werke* 3). For simplicity they concentrated on rectilinear boundaries (polygonal space curves), such as the one obtained from four edges of a tetrahedron, but eventually they claimed to be able to solve the problem for all space polygons. Their method was to find the second-order ordinary differential equation for the functions  $G$  and  $H$  above, and then to fit the corresponding minimal surface to the given boundary. This is a bold generalisation of the plane problem known as the Schwarz–Christoffel problem, but it is not clear that Schwarz or Weierstrass could provide a rigorous proof, and the full details were first given by Darboux in his (1887).

Independently, and very likely by 1860, Riemann had made a direct attack on the minimal surface problem (the published paper (1867) notes that Hattendorff edited from a manuscript consisting almost entirely of formulae that Riemann had discussed with him). This work led Riemann to a different form of the Weierstrass–Enneper formulae. He solved some simple cases of fitting a minimal surface to a prescribed space polygon, and then gave a difficult argument to show that differential equation for the general problem (any space polygon) can be solved.

Publication of his work gave rise to a long-rumbling priority dispute, in which Schwarz made the exaggerated, and never substantiated, claims on behalf of Weierstrass that were mentioned above and which, it now seems, were withdrawn by Weierstrass in lectures in 1883 (not mentioned in his *Werke*).

**Conclusions.** Some cases of the minimal surface problem, and of the constant mean curvature problem, were understood by mathematicians and by Plateau.

The minimal surface problem was solved mathematically by Riemann and Weierstrass–Schwarz in the 1860s, independently of each other and of Plateau’s work.

The Plateau problem – find the minimal surface that spans a given contour – was very poorly understood by the mathematicians, some of whom made exaggerated claims about what could be done.

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## Crystallography: models and mindsets

MARJORIE SENECHAL

R. J. Haüy's structure model for crystals, proposed in 1801 ([1], capped a century of efforts by mineralogists to explain crystal form. The essential feature of a crystal, he argued, is not its polyhedral shape (crystals of a single species might appear in different shapes), but a unique subvisible "nucleal" building block, copies of which that can be stacked to approximate different polyhedra. Since the number of blocks making up a crystal is, for all practical purposes, infinite, a crystal can be thought of as a tiling of three-dimensional space.

Haüy's tiling model was immediately successful ("tout est trouvé! he is said to have exclaimed) because it appeared to account for crystal growth as well as form. The model soon inspired the Bravais' classification of point lattices by symmetry and led, by century's end, to the enumeration of the three-dimensional crystallographic groups. Paradigm found: crystals were henceforth identified with periodic patterns and tilings. With the discovery in 1912 that crystals diffract x-rays, "long range order" also became identified with periodicity in the minds of mathematicians and solid state scientists.

Since 5-fold rotational symmetry is incompatible with periodicity in the plane or in three-space, icosahedral symmetry was thought (and taught) to be impossible for crystals. Yet, in 1981 aperiodic Penrose tilings were shown to have long-range order: laser light passing through a "mask" with pinholes at the vertices of a Penrose tiling gives a sharp optical diffraction pattern with five-fold symmetry [2]. The next year Dan Shechtman found icosahedral symmetry in the diffraction patterns of certain metallic alloys. Since this was "impossible," these alloys were quickly dubbed "quasicrystals." Quasi or not, the International Union of Crystallography soon expanded its definition of crystals to include them [3] and in 2011 Shechtman was awarded a Nobel Prize. Paradigm lost!

Yet the tiling model persisted as a habit of mind. Surely quasicrystal structures are some sort of 3-dimensional Penrose tiling? But it seems they are not. The first quasicrystal structure to be "solved" (25 years after Schechtman's discovery!) is not a tiling of any sort. Rather, it's a packing, with gaps and overlaps, of nested atomic clusters with icosahedral symmetry [4]. This suggests rethinking crystals models from scratch, creating a new geometry – the geometry of soft-packings – and ever-closer collaboration with solid state scientists [5].

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## **Role and Function of Visualization in Communicating Mathematics to a Larger Audience. Mathematical Instruments in the 17th-century**

### *Journal des savants*

JEANNE PEIFFER

In history of science, the field of visual studies has been one of the most active areas of investigation since the 1980s. Recent survey papers giving broad characterisations of the field claim that what is sometimes called iconic turn (G. Boehme 1994) is part of the practical or material turn in history of science. Adam Mosley understands thus texts, images and objects “as means of representation and persuasion, cognitive tools, and end-products of scientific labour” ([6], 292). Norton Wise [7] considers pictures embedded in scientific texts as arguments, Christoph Luethy and Alexis Smets [4] refer to epistemic images made with the intention of expressing, demonstrating or illustrating a theory, and Sven Dupré [2] speaks of carriers of knowledge. The knowledge-making role of pictures is investigated in a deluge of publications (see [3] for an important bibliography). According to Charlotte Bigg, research questions and methods of the field have been assimilated by the community of historians of science who are acquiring an overall “visual literacy” ([1], 99). Pictures’ long history of being ignored by historians of science comes to an end.

In history of mathematics, among the possible functions of images those concerning their heuristic role have been studied, suffice it to quote as an example Michael Mahoney’s thorough study of Huygens’ diagrams ([5]). Frontispieces have recently been analysed by Volker Remmert, the history of perspectival drawing (Kirsti Andersen, J. V. Field et al.) is well known, and the investigation of mathematical models and instruments has always included visual aspects. However, investigations of the interpretation given to images by those who make use of them are rare in the history of mathematics. The meaning of what an image displays is not wholly determined by its maker, but depends also on the conventions for making and understanding images and upon the interpretation of the beholder or user. What a text says, an instrument does, or an image displays, is not wholly determined by its creator, but depends upon the interpretation of others as well.

We can thus ask the question of what happens when mathematicians use diagrams and pictures while addressing, not their specialised fellow mathematicians, but a broader audience like that of early learned journals? This question is part of a more general research project *Cirmath* (Circulations of Mathematics in and by Journals: History, Territories and Publics, dir. by H. Gispert, P. Nabonnand, J. Peiffer, with the financial support of the French Agence Nationale de la Recherche),

the aim of which is to study questions concerning the circulation of mathematics at different scales and in different cultural areas, its main actors - producers of mathematics, popularizers, teachers, users, publishers, etc - as well as the editorial strategies and backgrounds. Our main hypothesis is that, while circulating from one cultural setting to another, the meaning of mathematical texts is likely to change, as well as the meaning of visual devices, like diagrams, graphs, drawings etc. The conventions for writing mathematics may be no longer shared and readers trained differently appropriate them differently.

Bruno Latour, in the 1980s, coined the term inscription, which abolished the border between text and image, inscription including the whole range of diagrams, symbols, tables, graphs, maps, trees, and other similar material. Even a different lay out may change the status of a text. The partial French translation of the *Philosophical Transactions* published from 1738 to 1761 in Paris copied exactly the lay out (same format, same lettering, same number of letters per line, same number of lines per page) of the prestigious *Mémoires de l'Académie royale des sciences*. The proceedings of a rival academy, the London Royal Society, are thus as highly valued as the Parisian one, while the translators claim that the *Journal des savants* has served as a model for the *Philosophical Transactions*.

Having completed (together with book historian Jean-Pierre Vittu) a database including all (circa 20.000) articles, i. e. book-reviews and memoirs, printed in the *Journal des savants* from its creation, in January 1665, to the end of the eighteenth century, I decided for this conference to search for “Instruments and machines”, a category we have introduced in indexing the entries of our database, and to have a look at the visual aspects of the contributions found under that heading. It is not an actors' category, and most of the instruments and machines were categorized by the journalists under the heading *Mathematici* or the very general heading *Supplementum ad bibliographiam*, which includes independent memoirs and no book-reviews. The first interesting finding concerns the period during which the journal published on instruments and machines: circa 70 percent of the contributions to the topic were published in the seventeenth century (before 1702 to be precise). The break can be explained by other publication projects of abbé Bignon, then editor of the journal, like the collection *Description des arts et métiers* at the Académie royale des sciences in Paris. During the seventeenth century, concrete models of machines were the privileged means of representation of machines, and not pictures or textual descriptions. According to its 1699 regulation<sup>1</sup>, the Academy collected and publicly displayed such concrete models.

Looking for the modes of presentation of machines and mathematical instruments in the *Journal des savants*, the main questions that I have explored concern

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<sup>1</sup>According to its art. XXXI: L'Académie examinera, si le roi l'ordonne, toutes les machines pour lesquelles on sollicitera des privilèges auprès de Sa Majesté. Elle certifiera si elles sont nouvelles et utiles: et les inventeurs de celles qui seront approuvées seront tenus de lui en laisser un modèle.

(1) the purposes for which mathematical instruments were published in the journal, and thus circulated among the journal's readership; (2) the audience implicitly addressed by the rhetorical devices, the technicalities, and the visual aspects.

At the hand of some examples from the *Journal des savants*, it was shown that the machines and mathematical instruments displayed on the pictures are represented ready to be used. The pictures show what will necessarily happen if the machine is correctly operated. This is for instance the case with the clepsydra (inspired from a fountain designed by Heron), which Claude Comiers presented to the Academy on April 11th, 1676, and published subsequently in the issue of May 11th, 1676, of the *Journal des savants*. While the text specifies that the machine had been built by "le sieur Hubin Émailleur ordinaire du Roy", and that Claude Comiers was the author of a booklet entitled *Nouvelle science* (1665) and then goes on describing the principle (gravitational potential energy) on which the device is built, and detailing the construction of the machine, the picture displays the clepsydra ready to be used. The reader is asked to activate the mechanism and to look at the effects of the machine. In this case, which is not an isolated example, the pictures seem to be substitutes for the concrete models that the inventors present to the Academy.

In another example, a trisector of an angle, published in the September 20th, 1688 issue of the journal, the instrument is said to have been invented by "Mr. Taragon, Professeur des Mathématiques à Paris", and the diagram is simply meant to be a proof (without any other explanation or demonstration), although the journalists recall the impossibility of the trisection of an angle. An oral commentary is necessary for the instrument to be understood. This short article advertises an instrument, the use of which can be learned with its inventor who teaches mathematics in Paris.

As a preliminary conclusion, the instruments and machines published in the 17th-century *Journal des savants* aimed at showing how the devices worked and at giving notice where they are shown and operated. They were addressed to a double audience:

(1) The mathematicians eager to know how to use these instruments and where to find them. The pictures give an idea of their accurateness and the expected effects.

(2) A learned audience who enjoys watching precisely the effects of these machines without being interested in the working principles. This audience may in many ways be comparable to the readers of the costly produced *Théâtres de machines*, even if the representations in the *Journal des savants* are but the pale shadows of these splendid illustrations.

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## Mathematics on Display: Mathematical Models in Fin de Siècle Scientific Culture

ULF HASHAGEN

Nowadays mathematical models and instruments are part of a former world that is alien to mathematicians in terms of their scientific culture and tradition. As a consequence these objects of a former material culture of mathematics can only be found in museum displays or gather dust in the old display cases of mathematical institutes – this is, incidentally, a remarkable example for the domestication of the past to serve present needs [10].

However, in the ‘long century’ before the First World War mathematical models, instruments and apparatus were important scientific tools in research and teaching in the sciences and also an important part of the scientific culture of mathematics [11]. An argument in support of this statement is that in the late 19th century mathematics became an exhibition subject. Since mainly mathematical models and instruments served as symbols for the public representation of mathematics, it seems appropriate to interpret ‘mathematical exhibitions’ as representations of *fin de siècle* scientific culture (with reference to [7] and [4]). In this context a ‘mathematical exhibition’ is to be taken literally as an exhibition of a ‘large’ collection of ‘mathematical artefacts’, which is addressed to the general public or to a larger part of the community of scientists and academics. This cursory essay is confined to a consideration of the basic historical questions of who created the mathematical exhibitions, when, where, why, and how by giving an overview of the mathematical exhibitions in the late 19th and early 20th century.

In the time period between 1870 and 1914, six ‘major’ mathematical exhibitions could be identified. Three exhibitions were located in Germany, two in Britain and one in the US. Four and two exhibitions were financed by the German and British Government respectively as well as curated by German and British mathematicians respectively. Moreover, one should mention that three of the exhibitions were independent ones, while the other three were part of a larger exhibition. The individual exhibitions were made on the occasion of national or international congresses or on the occasion of a science jubilee, while the other ones were part of a science exhibition, a world exhibition or a museum exhibition.

The first mathematical exhibition was shown as a part of the gigantic *Loan Exhibition of Scientific Apparatus* in London in 1876. More than 20,000 artefacts and

more than 250,000 visitors made this exhibition a major cultural event. Despite of its British nationalist agenda the exhibition propagated an internationalist culture of science, and especially Germany was represented by hundreds of exhibitors and thousands of artefacts. Mathematics was represented by sections on *Arithmetical Instruments* and *Geometrical Instruments and Models* including quite a large amount of artefacts from France and Germany. Remarkably German mathematicians were much more enthusiastic about the rich collection of mathematical models being shown at the exhibition than their British colleagues [5].

The first ever individual mathematics exhibition was planned by the mathematician Walther Dyck [6]. The *Exhibition of Mathematical and Mathematical-Physical Models, Apparatus and Instruments* was opened in 1893 in Munich on the occasion of the Annual Conference of the German Mathematician Society. Dyck gave the exhibition parts, which took four halls in the central tract of the Munich Polytechnic, the titles *Calculus*, *Geometry*, *Mechanics* and *Mathematical Physics* and dedicated them to Leibniz, Descartes, Galilei and Newton respectively. In the *Calculus* hall calculating machines, planimeters and harmonic analysers were shown, while in the *Geometry* section were mathematical models as well as geometrical instruments to be seen. In the *Mechanics* hall numerous apparatus for demonstrating laws of dynamics, statics and kinematics could be seen, while in the last hall on *Mathematical Physics* models that mechanically illustrated electrical processes, crystal structures, optics and thermodynamics were displayed [1, 2].

Dyck's opening speech about the relationship of the exhibited objects to mathematical instruction and research is a precious testimony on mathematical exhibitions in German "fin de siècle" scientific culture. Dyck distinctly conceded the boundaries and restrictions of this mathematical artefacts by explaining that although many of the models and instruments did not have a practical use they had an instructional purpose. Although he also stated that the exhibition could not claim to present the entire contemporary advancement of mathematical research, Dyck gave these artefacts and his exhibition an important symbolic significance for the advancement of the mathematical sciences in the 19th century. In his view the exhibition had its greatest value in stressing the close relationship between research in pure and applied mathematics being one of the most important moments in the development of mathematics [6].

A second mathematics exhibition was opened in 1893 as part of the German Universities' Exhibit at the World's Columbian Exposition. Although it was again Dyck, who had agreed to take on the organization of an exhibit of German mathematics on behalf of the Prussian Cultural Ministry, the exhibition clearly pursued other aims, namely to highlight the outstanding position of German mathematics in the world. In contrast to the Munich exhibition a colossal bust of Gauss and portraits of Dirichlet, Jacobi and Riemann served to celebrate the outstanding role of (German) scientists in the 19th century. While the collection of mathematical models and instruments exhibited was quite smaller than in Munich, the academic visitors of the exhibition should mainly be impressed by copies of some 500 books

by German mathematicians, a complete set of seven German mathematics journals and the publications of the four scientific academies in Germany [3, 12, 6].

It took more than ten years before the next mathematical exhibition was opened in Heidelberg in 1904 on the occasion of the international congress of mathematics. A committee of five German mathematicians had been responsible for the conception and organisation of an international *Exhibition of Mathematical Literature and Models*. While the collection of mathematical models and instruments exhibited was quite small and restricted to artefacts which had been added in recent years after the 1893 exhibition, the organizers of the congress claimed to have organized the first ever exhibition of mathematical literature by showing an almost complete collection of the mathematical publications of the last decade [9].

At almost the same time, *fin de siècle* mathematical artefacts became museum objects in Germany. It was again Dyck, who was responsible for the *Mathematics Exhibition* in the newly founded *Deutsches Museum* in Munich. Although mathematical models and instruments as well as portraits of important mathematicians were again central elements of the exhibition, in comparison with the 1893 exhibitions in Munich and Chicago this museum exhibition had a much clearer pedagogical impetus as well as a historical focus. For example, the principles of calculus as well as the development of computing methods and of geometrical research methods were shown on exhibition panels. One should mention here that the mathematical models, which had been central elements of the 1893 exhibitions, stayed much more in the background than mathematical instruments and calculating machines [6].

The opening of the *Napier Tercentenary Exhibition* in Edinburgh in July 1914 at the same time symbolized the end of the era of *fin de siècle* mathematical exhibitions. A few days before the outbreak of the First World War scientists from all over Europe celebrated the invention of the logarithms. In the exhibition besides antiquarian *Napier Relics* a collection of ‘mathematical artefacts’ was shown under subtitles *Mathematical Tables, Calculating Machines, The Abacus, Slide Rules, Other Mathematical Laboratory Instruments, Ruled Papers and Nomogramms* and *Mathematical Models* [8]. Remarkably British scientists again had made an exhibition concept with a pragmatic classification of the mathematical artefacts—similar to the 1876 exhibition in London—and did not present symbolically enhanced objects as their German colleagues.

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### **“Modelling Plasticity: Richards von Mises’ contribution, in particular his yield condition (1913)”**

REINHARD SIEGMUND-SCHULTZE

With his paper “Mechanics of solid bodies in plastically-deformable state”, published in the *Göttinger Nachrichten* in 1913, Richard von Mises (1883–1953) became a co-founder of the theory of perfect plasticity [1]. He rediscovered and explained equations of plasticity first published by the Frenchman Maurice Lévy in 1871 in an article which had been unknown to von Mises.

New and influential both in practical and methodological respect was von Mises’ “yield condition” for the transition of isotropic, ductile materials (such as metals) from the elastic to the plastic state, which modifies an earlier condition (1864) by the Frenchman Henri Tresca (1814–1885). Von Mises’ condition can be interpreted as an example of successful mathematical modeling in an engineering context. The condition depends only on the stress tensor, which describes the stress (limit of force per unit area around a point) which acts in any plane through a given point of the material. The strain (deformation) tensor and the rate-of-strain tensor do not contribute to the yield condition and come into the theory only as a next step, when the Lévy-Mises equations of plastic flow are discussed.

Several historians of the theory of plasticity have found von Mises’ contribution “purely mathematical” and lacking physical interpretation. However, von Mises’ paper [1] is heavily based on “empirical facts”, to which he devotes a central second section (“2. Erfahrungs-Grundlagen”) of his article. Among these empirical facts rank foremost the dominance of shear stress in the transition to plastic deformation and the negligibility of hydrostatic pressure. The latter fact leads to the reduction of the full stress tensor to the “deviatoric” stress tensor, after subtracting the arithmetic mean of the three diagonal elements from each diagonal element, thus resulting in a symmetric matrix with trace zero.

There is no doubt that von Mises’ article is “mathematical” primarily in the sense of “mathematical modelling”. From a purely mathematical point of view, von Mises’ theory and his paper [1] are not very technically demanding; everything is based on Cauchy’s notion of the stress tensor and the orthogonal transformation

of symmetric matrices. The real problems are on the conceptual level, the delimitation of liquid and solid, of viscosity and plasticity etc. However, one should not forget about von Mises' later numerical work (vector iteration), finding eigenvalues of symmetric matrices [2].

Personally I have the impression that von Mises is much better known among engineers and engineering students than among mathematicians, mainly due to his plasticity theory.

There are two relatively simple examples of stress state, which have served for the plasticity theorists as starting points for experiments and as "benchmark" cases for the evaluation of more complicated stress situations: the two states of stress are simple uniaxial tension (or compression) and pure shear.

For the engineers working in plasticity the problem was and is the following: Can one compare more complicated stress situations with the two (standardized) cases simple tension and pure shear? In particular, are the critical values of shear which occur in these cases of general importance? This would allow conclusions to be drawn from experiments for the standardized situations, in which one finds the "yield strength" depending on the material, and the results could then be applied to more complicated stress situations. As to experiments, it was known that experiments with simple (normal) tension or compression (similar to elasticity theory) are easiest to perform and to measure.

A basic empirical fact is that the maximum (absolute) values for shear components of the stress vector which can occur in any plane through a given point of the material, occur in planes tilted by  $45^\circ$  against the principal planes with pure normal stress. The latter planes exist due to the transformability of the symmetric stress tensor into diagonal form. Tresca and the German engineer Otto Mohr (1835–1918) assumed that plastic yielding occurred when the absolute value of shear contributing to the traction vector of stress with respect to any plane through the given point reaches a material-dependent constant  $K$ .

Von Mises was the first to represent the Tresca-Mohr condition for the elastic limit (yield limit) as a regular hexagon in the 3D-space with principal shear stresses (which occur in the tilted planes) as rectangular coordinates. The vertices of the hexagon represent the six possible configurations of simple tension (or compression) in a given point, while the middle points of the edges represent states of pure shear.

Von Mises now argued that existing experiments had been mainly based on simple tension (or compression), while mixed states of stress (those between simple tension and pure shear) had been rarely investigated or quantified. Therefore it should be permissible to replace the hexagon periphery by the periphery of the circumscribed circle as a yield limit. This has mathematical advantages, because, coincidentally, the second invariant of the (deviatoric) stress tensor (i.e. the second coefficient of the characteristic equation) can be written as a scalar multiple of the sphere-equation in the 3D-space of principal shear stresses.

It turned out in experiments that von Mises' "purely mathematical" yield condition predicts better values for the yield limits in mixed states of stress than the



Tresca-Mohr condition, coinciding only for simple tension. It is widely used in engineering applications today. Moreover, von Mises' "second invariant model" has theoretical importance in modern theories of plasticity and "rheology". Concerning the alleged lack of physical interpretation (because there is no simple connection to a physical condition such as a maximum shear stress) and the advantages of mathematical modelling, two leading experts in plasticity theory, William Prager and Philip G. Hodge, said in 1951:

"Actually, Mises' yield condition derives its importance in the mathematical theory of plasticity not from the fact that the invariant  $J_2$  appearing therein can be interpreted physically in this or that manner, but from the fact that it has the simplest mathematical form compatible with the general postulates which any yield condition must fulfill. The fact that it is also in reasonable good agreement with the empirical evidence regarding the yielding of structural metals must be considered as fortuitous: even if this agreement had been less satisfactory, the mathematically simple yield condition . . . would certainly have attracted the attention of those interested in the development of a general and yet workable theory of plasticity." [3, pp.26–27].

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### **Riemann and Nobili's rings: issues in modelling and verification**

TOM ARCHIBALD

Nobili's rings, discovered in 1824, are formed by the passage of a steady electric current from a point anode through a thin layer of electrolyte on a conducting plate, the cathode. The coloured rings that form on deposition of the electrolyte are due to optical interference when light passes through the thin film formed by the products of electrolytic decomposition. The phenomenon thus afforded an opportunity for precision measurement related to conduction, and inspired several attempts to create a mathematical model. The present paper aimed at a reconsideration of these attempts in the context of the history of using partial differential equations to model physical phenomena. It was a reconsideration in the sense that I published a paper on it some years ago, and wanted to look at the same material from a different point of view.

Georg Simon Ohm in 1826 gave an account of one-dimensional conduction, based on the model of Fourier for heat, using the same kinds of assumptions but with a different conception of what was going on physically from that which we

now employ. Ohm's electroscopic force, or electric tension, became identified by G. R. Kirchhoff in 1845 with a potential. Kirchhoff investigated conduction in three dimensions in the following years, developing sophisticated techniques that employed results on Bessel functions that had emerged in the late 1830s in the work of Kummer associated to his studies of the hypergeometric equation. These researches formed a direct background to the mathematical work of Riemann on Nobili's rings.

The diverse attempts at creating mathematical descriptions were due to Edmond Becquerel (1845), to Emil du Bois-Reymond and Wilhelm Beetz (1847), and to Bernhard Riemann (1855). It is anachronistic to describe such attempts as "modelling", though I will use the term occasionally in what follows. In particular, the issue of the extent to which such mathematical descriptions aim at an understanding of such things as physical reality or natural law are left more or less implicit in the papers concerned, though in the case of Riemann we do have some statements about how he viewed such mathematical efforts in that regard. I return to this below.

Coming as they did from people with widely differing degrees of involvement with mathematics, the models provide a good point to assess the various approaches to such phenomena that were in play in the middle years of the nineteenth century. They also shed interesting light on Riemann's background in what was to become in essence his first journal publication.

Writing in 1845, Becquerel assumed that the electrolyte deposition was proportional to current (Faraday's law, as he termed it) and that the current was proportional to the distance from the point electrode. This ad hoc assumption led him to a thickness inversely proportional to the distance from the point electrode, and he found this to be verified by the sequence of colours of the rings and the interior and exterior diameters of the individual rings.

Shortly thereafter, Beetz and du Bois-Reymond in Berlin created their own mathematical description. noting in an 1847 publication in Poggendorff's *Annalen der Physik* that Becquerel should have used Ohm's law, and should not have assumed rectilinear propagation since the current must move perpendicular to the equipotential surfaces. These authors were thus using the version of Ohm's law recently made familiar by Kirchhoff. Beyond this, however, the level of mathematical sophistication is limited. They deduced an inverse-cube law, and checking it against previously tabulated data on interference at various wave-lengths found it to be in excellent agreement with observation.

These three writers are rooted in experimental science, with Becquerel carrying on his father's work in electrochemistry and Beetz and du Bois-Reymond as experimental physicists in the tradition of Magnus in Berlin. Du Bois-Reymond's long career in electrophysiology lay ahead of him at that point.

Riemann, on the other hand, had a firmly mathematical training. His inculcation in mathematical physics had come at the hands of the two mathematicians who are usually seen as his main influences, Dirichlet and Jacobi, whose lectures he had followed in Berlin in 1848. In the period of time leading up to and following his

Habilitation in 1854, Riemann worked as Assistant to Wilhelm Weber, and became deeply interested in physical problems at this time. From Riemann's letters to his family, published already by Dedekind in his biographical account, we know that he hoped to create a unified physical theory for gravity, light, electricity and magnetism. We also know that he had contact with Weber's Berlin colleague Rudolf Kohlrausch in this period. Apparently encouraged by Kohlrausch and Weber, Riemann arranged to give a paper to the 1854 annual meeting of the *Vereinigung deutsche Naturforscher und Ärzte* on a topic in electrostatics. Kohlrausch in fact arranged with Poggendorff to publish a paper on this work in the *Annalen*. As it turned out this appeared only in the proceedings of the meeting.

However, Riemann did produce a paper on Nobili's rings which was published by Poggendorff in 1855. In this work the problem is formulated in a way that looks very modern, namely, in a tableau consisting of a partial differential equation with listed side conditions, each of which is clearly linked to a feature of the physical problem. This style of presentation doubtless derived from Dirichlet, but became standard in part through the publication of Riemann's lectures on partial differential equations, which had a long afterlife as Riemann-Weber-Frank-Mises. [1]. This paper uses a broad arsenal of relatively new mathematical techniques: Fourier analysis, the Cauchy techniques for finding integrals of functions of a real variable, asymptotic expansions, and Bessel function representations due to Kummer. The result of Riemann is different from that of Beetz and du Bois-Reymond, which led Beetz to repeat his experiment and agree that the version of Riemann did indeed give better agreement.

Despite his own proximity to the best experimentalists and excellent laboratory facilities, Riemann apparently did not attempt to carry out his own experiments on this matter; in any case, such results were not published and they have apparently left no trace in his *Nachlass*. This is typical of mathematical physics in Germany in this time period, as I have discussed elsewhere. When comparisons are made, they are usually to earlier theoretical work.

Riemann's conception of what he was engaged in is consistent with the picture that he was to provide in the introduction to his *Vorlesungen*, written between 1856 and 1861 and published in 1876 by Hattendorff. He does not emphasize measurement or experiment as such, but rather presents his efforts as part of a "scientific physics" which attempts to reconstruct the connection of phenomena into abstract concepts. The latter are of two kinds: the simple basic concepts with which we construct the physics (here he states explicitly that he refers to Galilean and Newtonian laws of motion); and the laws or rules for times and distances that are accessible to observation. This is not modelling in the modern sense, even if it employs almost the full range of tools that we now set at the centre of mathematical modelling.

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**Models and visual thinking in physical applications of differential equation theory: three case studies during the period 1850–1950 (Bashforth, Størmer, Lemaître)**

DOMINIQUE TOURNÈS

This paper is organized around three important works in applied mathematics that took place in the century 1850–1950: Francis Bashforth (1819–1912) on capillary action [1], Carl Størmer (1874–1957) on polar aurora [4], Georges Lemaître (1894–1966) on cosmic rays [3]. I have chosen these three figures for several reasons: they were applied mathematicians with strong theoretical training; they studied complex physical problems for which they had to create new numerical methods at the limit of the human and technical possibilities of their time; there is a natural continuity in their works, each being partially inspired by the previous one; finally, these works present the same characteristics as what we call today mathematical modeling and computer simulation.

Francis Bashforth was fellow at St. John’s College at Cambridge and later professor of mathematics at the Royal Military Academy of Woolwich. Between 1864 and 1880 he developed important experimental and theoretical research on ballistics. Before and after his professional engagement in artillery, he was also interested in capillary action. In this domain, his major aim was to compare the measured forms of drops of fluid resting on a horizontal plane, obtained by experiment, with the theoretical forms of the same drops as determined by the Laplace differential equation of capillarity.

In his research, Bashforth used, on the one hand, a new measurement process involving a micrometer of his invention and, on the other hand, a new method of numerical integration of differential equations involving finite differences of the fourth order and efficient quadrature formulas, conceived with the help of the famous astronomer John Couch Adams [5]. Bashforth, with his assistants, computed 32 integral curves, each of them with 36 points. Knowing that five auxiliary values were necessary for each point of the curve, we arrive at the total of more than 5000 numbers to be calculated. The calculation time can be estimated to at least 500 hours. The coincidence of the curves obtained by the experimental method and the numerical one was excellent and could be viewed as a mutual validation of the two approaches of the given capillary problem.

In Bashforth’s work, we may distinguish different levels of representation of the physical phenomenon concerned. Experimentation and measurement lead to what I call an “experimental model” of the forms of drops. In parallel, the mathematization of the problem gives birth to what we would call today a “mathematical model.” This model is non-operative because we cannot integrate the differential equation analytically, so it is necessary to discretize this equation to obtain a “numerical model”. This process of discretization is not a simple translation. It would be an error to consider the continuous mathematical model and the discrete numerical model as being obviously equivalent. In fact, a discretization process often introduces significant changes in the informational content of the original model, because a numerical algorithm may be divergent, may suffer

from numerical instability, and may be unadapted to the available instruments of calculation.

Carl Størmer, the second character in my story, was a Norwegian mathematician trained in Kristiania, Paris and Göttingen. For many years until his retirement, he was professor of mathematics at Kristiania University. Up to his death, the major part of his research was devoted to the study of the curious phenomenon of polar aurora, called also “aurora borealis” or “northern lights”, on which he published almost 150 papers.

Understanding that polar auroras are caused by electrically charged particles coming from outer space, Størmer decided to determine the trajectories of these particles under the action of terrestrial magnetism. In order to track these trajectories step by step from the Sun to the Earth, he had to develop new techniques of numerical integration of differential equations, inspired by those of Adams-Bashforth and British astronomers, but best suited to his specific problem. With his students, he calculated a multitude of different trajectories during three years. He himself estimated that this huge task required more than 5000 hours of work.

After that, Størmer and his assistants constructed several wire models to visualize the numerical tables issued from the calculations. These material models showed that the charged particles coming from the Sun concentrate around the polar circle, in accordance with observation. These models also explained in a convincing way why the northern lights can appear on the night side of the Earth, at the opposite of the Sun.

A few years before, a Størmer’s colleague, Kristian Birkeland, professor of physics at Kristiania University, had realized a physical simulation of the polar aurora. For that, he was sending cathode rays through an evacuated glass container against a small magnetic sphere representing the Earth, which he called “terrela”. Birkeland’s simulations showed two illuminated bands encircling the poles, in agreement with the behavior of northern lights and also with the computed trajectories obtained later by Størmer.

Finally, the physical phenomenon of polar aurora has been studied by three ways. First, by direct observations and measurements, secondly by Birkeland’s simulation, which we can consider as an “analog model”, and thirdly by Størmer’s mathematization with a continuous mathematical model consisting in a system of differential equations, a numerical model obtained by discretization and a wire material model representing concretely the trajectories. The coherence of the results obtained by these three approaches validates strongly the initial hypothesis of charged particles deviated by terrestrial magnetism.

My third and last part is devoted to the astrophysicist Georges Lemaître and his research on cosmic rays. At this time, an important problem addressed by Millikan was to explain the origin and nature of the cosmic rays detected by balloons or mountain observatories. There were two rival conceptions of these cosmic rays, one principally advocated by Millikan and the other by Arthur Compton. While Millikan held the rays to consist of high-energy photons, Compton and his collaborators argued that they were charged particles of extragalactic origin. Lemaître

was interested in these cosmic rays because he saw in them the fossil traces of his “Primeval Atom hypothesis”, an ancestor of the Big Bang theory, so he wanted to prove the validity of Compton’s conception. In collaboration with the Mexican physicist Manuel Sandoval Vallarta, Lemaître engaged in complicated calculations of the energies and trajectories of charged particles in the Earth’s magnetic field.

At first, Lemaître and Vallarta tried to integrate numerically the differential equations of the trajectories with the Adams-Bashforth method, but this was not convenient. Later, they discovered the Størmer method in the literature and began to use it, but the calculations were very tedious to perform. Finally they thought of the differential analyzer constructed by Vannevar Bush at the MIT [2]. A differential analyzer is a mechanical analog machine conceived for the integration of differential equations. It is constituted by algebraic mechanisms that perform the algebraic operations and mechanical integrators that realize the integrations. Once suitably prepared, the machine is in exact correspondence with the given differential equation and when it moves from an initial given state, it traces exactly an integral curve of this equation.

For the use of the differential analyzer, Lemaître and Vallarta were helped by Samuel Hawks Caldwell, an assistant of Bush who managed the differential analyzer for the specific problem of cosmic rays. Thanks to this instrument, they could obtain hundreds of trajectories within a reasonable time. In this third situation, we find again the notions of experimental, mathematical and numerical models already analyzed in Basforth’s and Størmer’s researches, but the novelty is in the role played by the differential analyzer: this instrument being a mechanical analog model of the differential equation, it appears also, indirectly, as an analog model of the physical phenomenon of cosmic rays.

In the three situations we have studied, we encountered several representations – experimental, analog, mathematical, numerical, graphical, material – of a physical phenomenon that validate each other through the consistence and coherence of their results. Each of them brings specific information about the real phenomenon. In fact, these representations make sense when they are considered together, so I am tempted to say that this is this system of representations considered as a whole which constitutes a “model” of the phenomenon. Concretely, we can only reason and calculate in this multifaceted model, whereas the reality of the phenomenon remains definitively hidden.

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## Geometric Intuition in the Work of Marcel Grossmann

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Modern general relativity is a geometrized physics. Surprisingly, geometric intuition played little if any role in the genesis of the theory with Einstein and his friend and co-author Marcel Grossmann. The latter explicitly considered the use of geometric aids as not helpful when providing his adapted version of the absolute differential calculus as the mathematics of choice in their search for a generally covariant field equation. Grossmann signed responsible for the mathematical part of the jointly authored *Outline of a Generalized Theory of Relativity and a Theory of Gravitation* [1], Einstein signed responsible for the physical part. This division of labor makes the *Outline* a prime example for a study of the relationship between mathematics and physics and for the applicability of mathematics in the real world [2].

In the introductory section to his part, Grossmann identifies the mathematical tradition from which he takes his clues as that of Bruno Elwin Christoffel as well as of Gregorio Ricci-Curbastro and Tullio Levi-Civita. He also mentions the vector analytic works by Hermann Minkowski, Arnold Sommerfeld, and Max von Laue. Based on these authors, Grossmann set out to develop his own brand of tensor calculus, tailored specifically for the task of formulating a generalized theory of relativity. He wrote:

Since more detailed mathematical investigations will have to be done in connection with Einstein's theory of gravitation, and especially in connection with the problem of the differential equations of the gravitational field, a systematic presentation of the general vector analysis might be in order. [1, p. 244]

He added the remark:

I have purposely not employed geometrical aids because, in my opinion, they contribute very little to an intuitive understanding of the conception of vector analysis. (ibid.)

Indeed, a closer look at Einstein's notes from the so-called Zurich Notebook, documenting their joint work on the problem of gravitation prior to the publication of the *Outline* [3], shows no trace whatsoever of any geometric interpretation of the tensor expressions, let alone of any graphical representation or images. The rich geometric implications of the general theory of relativity were only realized after the advent of the theory by such mathematicians as Tullio Levi-Civita and Hermann Weyl.

This explicit renunciation of geometrical aids by Grossmann is even more surprising considering the fact that Grossmann was a geometer who made extensive use of graphical representation in his teaching and in his research [4, 5]. Already

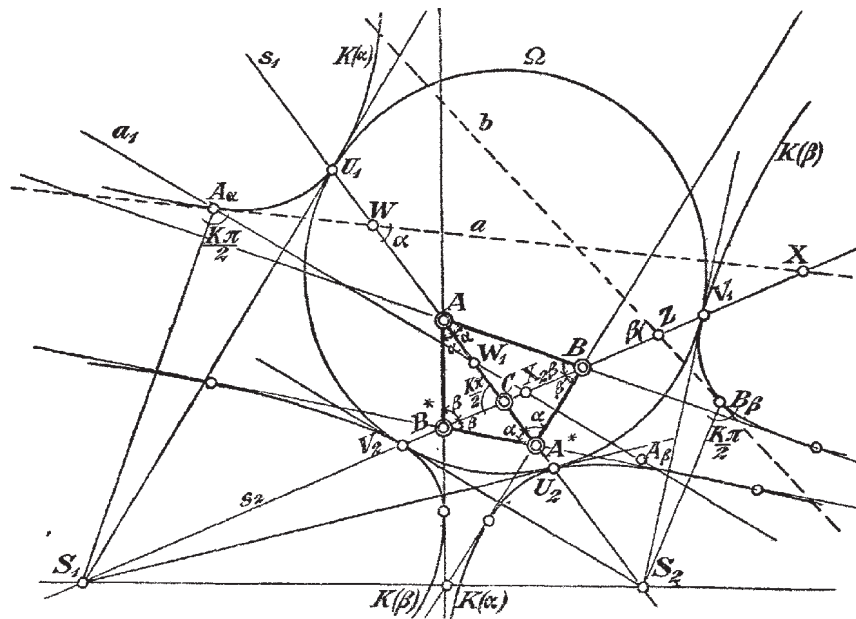


FIGURE 1. Construction of a triangle  $ABC$  from three given angles in the hyperbolic plane. The hyperbolic plane is represented by the inside of the conic section  $\Omega$ . [6, p. 579].

as a student at the Swiss Polytechnic in Zurich, Grossmann chose the mathematical track of the school for teachers in mathematics and physics, and he studied in particular with Otto Wilhelm Fiedler, who held the chair for projective and descriptive geometry in Zurich. With Fiedler, Grossmann took his Ph.D. in 1902 with a thesis on metric properties of collinear ray bundles. This early work was followed two years later with an investigation of the fundamental constructions of non-Euclidean geometry [6]. This work extended a tradition of descriptive geometry in the Euclidean framework to the non-Euclidean case of hyperbolic and elliptic geometries and relied heavily of graphical constructions which after all were their explicit aim. The constructions were based on the idea of employing the Cayley-Klein metric (defining the distance of two points in the plane as proportional to the logarithm of their cross ratio) and constructed triangles and other figures inside a conic that represented, say, the hyperbolic plane. With Fiedler's health deteriorating, Grossmann first took over his teaching and later became his successor as professor for descriptive geometry at the Zurich polytechnic in 1907. As a teacher, Grossmann trained generations of mathematics and engineering students at the ETH, and he wrote several textbooks on descriptive geometry. In contrast to the bulky and detailed monographs of his teacher Fiedler, Grossmann's textbooks were slim, concise, to the point, and written with the practical needs of the education of engineers in mind.

In his research, too, visualization and explicit construction was a central concern. Here are some examples. A typical problem, that would catch his attention was the problem of photogrammetry, i.e. the problem of reconstructing three-dimensional structures from several two-dimensional projections (photographs)



[7]. He also gave a geometric construction of the horopter curve, important in the theory of binocular viewing, with elaborate graphical illustrations [8]. Another problem was directly relevant for the engineering practice. Grossmann analyzed the kinematics of roller and cam in a loom and found that geometrically the two surfaces were developing along a common line [9]. This observation was the basis for a scheme to produce cams for looms which, in 1928, was granted a patent, both in Germany and in Britain. Grossmann set out to build proof-of-principle prototypes of his machine and even had obtained funds for this project. Unfortunately, symptoms of an advanced case of multiple sclerosis made it impossible for him to pursue this project and forced him into early retirement. Grossmann died after many years of illness in 1936 at the age of 58.

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### Far from modelisation: the emergence of model theory

JOSÉ FERREIRÓS

The emergence of model theory, in the strict sense of the subfield of Mathematical Logic (03Cxx in the MSC), is a chapter in the history of pure mathematics in the mid-20th century. As a matter of fact, the name itself was only used from the 1950s: in 1954 Tarski announced “a new branch of metamathematics” under the name of the “theory of models” – first time that it was employed prominently.<sup>1</sup> Indeed, the classic textbook Chang and Keisler (1973, [1]) reads: “Model theory is a young subject. It was not clearly visible as a separate area of research in mathematics until the early 1950s.” That was a decade of intense development, and around 1960 came the time of Robinson’s non-standard analysis, which attracted

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<sup>1</sup>It is worthy of note that Tarski gave an invited speech at the 1950 ICM that was held in Harvard; the topic was indeed ‘model theory’ but the name is absent.

a lot of attention to this new subfield. Chen Chung Chang and H. Jerome Keisler were in fact two men of the powerful Berkeley school, established by Alfred Tarski from 1946.

In this paper, I shall offer some considerations about the early days of model theory leading up to the 1950s. The spirit of my historical reconstruction is close to the following words of logician Georg Kreisel: “the passage from the foundational aims for which various branches of modern logic were originally developed to the discovery of areas and problems for which its methods are effective tools... did not consist of successive refinements, a gradual evolution by adaptation..., but required radical changes of direction, to be compared to evolution by migration.”

All styles of model theory rest on one fundamental notion, namely the notion of a formula  $\phi$  being satisfied in a model  $M$  under an interpretation  $i$ . One may also speak of the truth of  $\phi$  under the interpretation  $i$  in model  $M$ . The classic treatment of these notions, with a very precise set-theoretic definition of truth for formulas  $\phi$  in a formal system (based on satisfaction) is in Tarski’s paper (1935).<sup>2</sup> Logic in the 1920s was conceived as dealing with strictly formal, syntactic notions. It came as a surprise that such “semantic” concepts could be mathematised.

**Problematising the insider’s history.** Specialists in the field typically emphasize that the first model-theoretic result came quite early in the century. In 1915, Leopold Löwenheim proved a theorem that would be subsequently improved by T. Skolem, the Löwenheim-Skolem theorem: If a sentence in the language of the first-order calculus has an infinite model  $M$ , then it has a countable model.<sup>3</sup> The interest of the result for Skolem was mostly in its foundational implications, the famous Skolem paradox: The formalized system of axiomatic set theory ZFC has a countable model. He thus argued that set-theoretic notions are relative, not an absolute foundation for math; Skolem remained a critic of the ZFC axiom system as logically and methodologically deficient.

Another important step was given by Kurt Gödel in his 1929 dissertation [3], proving the completeness theorem for the “restricted functional calculus” a.k.a. first-order predicate calculus (the logic FOL). Completeness implies that every consistent axiom system in the language of FOL has a realization, a model – either the system is inconsistent or there is a “Modell”. As a corollary of this key result Gödel obtained the Compactness theorem: “For a denumerably infinite system of formulas [of FOL] to be satisfiable it is necessary and sufficient that every finite subsystem be satisfiable.” ([3], 119) This would later become a central result of model theory – but notice that the name came much later; this name, and the new uses of the result after 1945, reflect a rethinking of this whole area of

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<sup>2</sup>Gödel too [3] underscored “the statement ‘A system of relations satisfies a logical expression’ (that is, the sentence obtained through substitution is true)”, where a “system of relations” is a model or structure.

<sup>3</sup>Löwenheim, ‘On possibilities in the calculus of relatives’, Math. Annalen. Skolem, ‘Logico-combinatorial investigations on the satisfiability or provability of mathematical propositions’, Kristiania Videnskapselskapet. There is a very interesting analysis and reconstruction of Löwenheim’s work in Badesa 2004.

questions in the light of abstract structures, establishing connections with topology and algebra.

Alfred Tarski obtained important results starting from a Seminar he held in Warsaw, 1926/28. This seminar was devoted to studying, making precise, and extending a method that had been employed by Löwenheim, Skolem, and Langford, which was called quantifier elimination. Applied to a formalized axiom system, the method yielded a description of all the relations definable by first-order formulas, an axiomatization of the set of all true first-order sentences, and an algorithm for testing the truth of any sentence. To solve decision problems was one of the key aims; Hilbert and Ackermann emphasized the Entscheidungsproblem as “the fundamental problem of mathematical logic”. Tarski obtained particularly striking results concerning an axiom system for the field  $\mathbb{R}$  of real numbers (a system which avoided notions not expressible in the first-order calculus, i.e. set-theoretic concepts like least upper bound).<sup>4</sup>

During the 1930s, the famous series of papers on “scientific semantics” were published, dealing with the notions of truth, implication or logical consequence, and definability. Tarski explored the topic in both its formal logical and its philosophical sides – here he was a clear product of the Warsaw school, with its combination of logic, philosophy, set theory and topology. In 1939, Gödel started the advanced model theory of ZFC with his famous work on the consistency of AC and CH. This was to fructify after the II World War, a period in which model theory and set theory became increasingly involved with each other. And this blend was another characteristic trait of the Berkeley school in Logic headed by Tarski.

Yet in spite of all this, and against insider views, I want to argue that Model Theory, in some crucial sense, does not appear until after WW II. What we have before is logical questions linked to issues in foundational research, with the problems of consistency, completeness, and above all decidability as the central ones (not elementary equivalence, nor compactness, etc.). There was no clear sense of a theory of models, just an increasingly rigorous employment of set-theoretic models and formal axiom systems, in the service of foundational questions à la Hilbert.

For the emergence of Model Theory some new moves and goals were required. It required a confidence in the general notion of a model (any structure of any cardinality) which is quite far from the perplexed syntactically-oriented considerations of the Inter-War period in Europe, marked by the tension between classicists and intuitionists. The new spirit included a free exploration of advanced set theory, and also the shift of focus from foundational aspects (axiomatics, decidability, etc.) to properly mathematical ones. Notice that the situation before 1940 was marked by a tense atmosphere, not only in politics but also at the level of foundations, while the situation after 1945 was one of freedom.

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<sup>4</sup>However, the story of the publication was very involved: he intended to publish a book in 1939 in France, *The completeness of elementary algebra and geometry*, but this was impossible due to the War; only in 1948 was a report published by the Rand Corporation, and made public two years later: *A decision method for elementary algebra and geometry*. Notice how the titles emphasize Hilbertian questions.

Those themes are visible in many ways. In the Inter-War, we find the importance of philosophical considerations (the Unity of Science movement, Vienna-circle empiricism, “logical syntax”); after WW II, now in the USA, philosophy played a much lesser role and mathematical goals become the focus. Witness the first book by A. Robinson, *On the metamathematics of algebra* (1951), and the talk given by Tarski at ICM 1950: ‘Some notions and methods on the borderline between algebra and metamathematics’. For Tarski, this was the time of endless new possibilities opened by his work at UC Berkeley (see [2]). Here he had many students (Vaught, Chang, Keisler, Montague, etc.) and colleagues visiting from all over the world, with whom he established the new subfield of logic.

Thus the transplantation from Europe to the USA seems to have been crucial, as well as the winning of the battle against intuitionism (won partly on the professional arena, not the foundational field; but likewise important was Gödel’s 1939 work establishing the reliability of AC and the CH). With the turn from foundational concerns to more purely mathematical ones came a reconception of model-theoretic methods, which now became tools in the study of structures.

In all this, Tarski’s vision of logic, emerging from logicism, played a central role; with the implicit (but central) idea that everything is done inside logic, that is to say, formalized set theory – the syntax, the semantics, the metatheory, the methodology of deductive sciences, and so on – in short, the foundations of rational thinking.<sup>5</sup>

However, while the interactions between model theory and advanced set theory were very strong, and its importance inside mathematical logic rose steadily, the promise of becoming a methodology of choice in abstract maths did not materialise so quickly. The situation seems very different since the 1990s due to powerful results obtained by Hrushovski, Wilkie, etc.

The whole area of Model Theory emerges very far from modelisation, in realms having to do with pure maths in some of its most characteristic, modernist expressions. This is mathematical logic influenced by abstract algebra and general topology. The question of the foundations of mathematics, of metamathematics, gradually gave rise to a specific subarea of maths – Model theory is the quintessential example of this double reflective turn. As John Baldwin says, “Model theory is the activity of a ‘self-conscious’ mathematician.” The informal work of the practicing mathematician becomes formalised, thanks to:

- (1) strictly formal axiom systems (e.g. Skolem with ZFC in 1923; Gödel and Tarski with simple type theory in 1930);
- (2) precise set-theoretic definitions of structures (e.g. Bourbaki, Tarski); and
- (3) precisely formulated notions of interpretation, satisfaction, truth.

But this was not enough for the project of Model theory, which required several moves in the mathematisation of metamathematics. We might speak of a paradigm shift in the 1940s led by Tarski.

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<sup>5</sup>His *Introduction to Logic and the Methodology of the Deductive Sciences* aimed “to present to the educated layman... that powerful trend... modern logic... [which] seeks to create a common basis for the whole human knowledge.”

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